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THIRD CONFERENCE, RICHMOND, SURREY, ENGLAND
6-8 AUGUST 1978.

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SYSTEM RESEARCH LTD

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The Third Richmond Conference centered on foundational problems in decision-making. The conference is reported in two volumes. Volume I contains an overview of each of the major papers presented, with the author's commentary and the discussion by conference participants, plus several general discussions. Volume II (ARI Research Note 80-11) provides the major papers themselves: Observable Components of the Decision Process and a Revised Theoretical Position, by Gordon Pask; Decision Making as an Event-Search: Traffic on a Multidimensional Structure, by R. H. Atkin; Decision: Foundation and Practice,

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by Brian R. Gaines; Competing Modes of Cognition and Communication in Simulated and Self-Reflective Systems, by Stein Braten; On the Spontaneous Emergence of Decision Making Constraints in Communicating Hierarchical Systems by John S. Nicolis; and also a paper by Maria Nowakowska on a new model of decision under risk.

CURRENT SCIENTIFIC APPROACHES TO DECISION MAKING IN COMPLEX SYSTEMS: III

Report of a Conference held at Richmond, Surrey England August 6th, 7th and 8th, 1978.
under the auspices of the
U S Army Research Institute for the Behavioural and Social Sciences and the
European Research Office, U S Army, London England

In two volumes: Vol I , Conference Proceedings

FINAL TECHNICAL REPORT

by

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CURRENT SCIENTIFIC APPROACHES TO DECISION MAKING IN COMPLEX SYSTEMS III. Vol. I, Conference Proceedings

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INTRODUCTION.

The main theme of the Third Richmond Conference was the area of foundational problems in decision making. This theme was approached from many different angles, positions, and points of departure. Agreements reached in the course of discussion functioned not only as conclusions, but as "lenses" through which the points of departure were seen afresh. In this sense a linear presentation of the proceedings does them less than justice. Like Finnegan's Wake, the presentation should be circular; the reader's point of entry being determined by his interest rather than the necessity of numbering pages in order, from "1" onwards. However this is impracticable.....

To try and capture the flavour of the proceedings, and to enable the reader to pursue his own interests within them — to allow him to enter as participant rather than spectator — a dual method of presentation has been attempted. We have presented the Seminal Papers in a separate volume, and collected the authors' commentaries, and discussion in the order in which they happened, in this volume. We have also provided an overview, preceeding each commentary. Seminal papers and discussion may be read in isolation, as statements in themselves, or in sequences, as part of a developing discussion, or in thematic sequences, as a perspective on a theme.

A major theme of the Conference was the importance of interacting perspectives, whether social or personal, in decision practice and in approaching foundational problems of theory. No single, or simplistic, definition ever suffices - not because it is wrong in itself - but because it fails to take account of the mobile topology of decision space. Yesterday's solution is tomorrow's problem. Perspectives are not only presented as points of view, agreements, and themes, but as self illustrating structures within the report.
In order to achieve this (and for more obvious reasons of space-saving) severe condensation and editing has been necessary. We have also introduced "Thematic summaries" after each section.

Section 1:

An Approach to Decision Making

Section 1

Overview

Dr Johnson outlined the ARI concern for several aspects of decision making, namely basic research, specific advanced development and a type of advisory role, in which the ARI is approached to provide an answer, rather than the (right) answer to various questions. In this application-context, the 1st European Conference was chiefly addressed by professional "decision theorists", the 2nd was oriented to training, and the present conference is structured to fill certain gaps left out in the debate. Often these gaps are filled by the work of people who are not "officially" decision theorists but are involved in the general field of rational action, based upon known features of a situation or specifically acquired information.

There is a tendency for psychologists to use "Decision Making" as the descriptor for any behaviour that cannot be given an unambiguous title: the tendency is defensible only so far as decision making is ubiquitous, it pervades learning, problem solving, and the like. But, in this case, some of the presupposed paradigms of decision making are unsatisfactory.

Perhaps the most deeply ingrained paradigm is "choice amongst alternatives". In complex systems, at any rate, it is wise to ask where the "alternatives" come from, how are they constructed or reconstructed, why do they exist, and is all decision making a choice amongst alternatives (for the alternatives are seldom given as they would be in the laboratory)!

If, for one reason or another, there are legitimate alternatives then attempts to express the decision process in terms of Bayesian extrapolation and multi-attribute utilities are not particularly useful at the level of complex command and control. Again, although studies of systematic mistakes and biases of judgement (illusory correlations, for example) have considerable value, it seems likely that a more fundamental appraisal of decision is required.

One novel paradigm is "Decision Aiding" , that is "Techniques or procedures that restructure the procedure used for problem solving (including an allocation of effort to the people and the computing machinery employed in decision making)".

Within this framework it may be recognised that decisions are *made*, *theory* or not, and that decision should not be too much inhibited by (unavoidable) ignorance. It is also clear that any model which is a candidate theory must represent the task or situation as it is seen by the decision maker, as well as representing the decision maker. Finally, the disposition to image decision making as sequential is frequently misleading for many decision processes more closely resemble a series of many potential sequences.

Commentary

Johnson: I would first like to make some comments on how this conference came about, and why the Army Research Institute should be sponsoring it. The Institute itself is the American Army's agency in the social and behavioural sciences. We are concerned with personnel and management; education and training; and human factors in systems development. In these areas, we work at three different levels. We have a basic research program, of which this conference is a part. Here we are concerned to develop the state of the art in areas that have general relevance for our concerns.

The second, and largest part of our program is concerned with advanced development and user problems. Here we work with very specific problems, and often come up with the fact that we don't know enough to begin with — again this leads us back to the relevance of this conference. The third aspect of our work is the technical advisor service. Various people within the service come and ask questions, and they are not necessarily looking for THE answer, only AN answer. In the area of decision making and decision process, it is difficult to provide an answer with which one is comfortable. People take actions based on the kind of advice one gives, and there are real-world consequences. In doing any kind of successful applications research, we are dependent on basic research and basic theoretical frameworks for the underlying processes.

This is the third conference, so I should say a few words about its genesis. The First Conference was held in 1975, was quite general, covered all facets of decision, and provided a rough picture of the state of the art. The Second Conference was more structured, and focussed on a somewhat overlooked aspect of decision making — training. How do you train people to become better decision makers? In both conferences there was a significant interchange of ideas and cross-fertilization of research — there were also some notable gaps, areas which were not represented. For this third conference, we have tried to invite individuals who could report on these areas. I have provided some questions that might be a useful point of departure for our discussion.

I think these are simple questions, but I also think simple questions are the most difficult to answer. They are questions which I would not feel comfortable in answering, but they are also questions which I get asked to answer - which is why I find them easy to ask.

Orienting Questions

1. What is decision making? How do you view the decision process and are there multiple perspectives?

2. What is a decision task and how can/should such tasks be structured?

3. The individual is often overlooked in analyses and theories of decision. How do you define the role of the decision maker? What systematic individual differences and variables should be included in a theory of representation of decision making?

4. Complementary approaches to improving decision include training, aiding and job/task design. What are appropriate "target variables" and how can they be evaluated?

5. How do you define "good decision", or "good decision making"? What are appropriate evaluation procedures?

6. What are the major unresolved issues in the development of descriptive (or prescriptive) theory (ies) of decision?

My next remarks are an attempt to integrate several threads from previous conferences, together with current issues. The result is somewhat disjointed, and abounds with speculation, dogmatic assertion, and inconsistencies. The intent is not to resolve issues, but to provoke discussion.

Decision making is a pervasive concept, a "wastebasket" of psychology. If you don't know what to call a piece of behaviour, you call it decision making. An issue which arises very early in any discussion of decision making is that of defining the class of phenomena under discussion and of identifying the parameters. In lieu of a definition which will be addressed during the course of the conference, I offer a number of assertions which circumscribe the domain (Table 1)

A decision is a selection among alternative actions and implies the allocation of resources. Action implies the commitment of resources or implementation of an alternative.

Decision is task specific. No decision takes place in a vacuum. Decision making involves (of necessity) a task as well as a decision maker.

Decision making is a process not an act.

Decision is goal oriented and must be described in reference to goals.

Decision should be distinguished from response mode (eg. selection or confidence estimation) and from habit following.

Decision making can be regarded as:
(a) a collection of cognitive processes/skills; or
(b) the process of converting information into action

Human beings are involved in any decision, although other elements may participate

Table 1 Assertions concerning Decision

Despite the problem of defining without ambiguity what is meant by decision, I have a general feeling that:

* Decision considered simply as a choice among alternatives is not a "useful" paradigm for improving decision. Yet this is the way most books define the subject. I think this avoids most of the interesting issues in decision making. Where do the alternatives come from? Why did someone create alternatives? How do you construct alternatives? Is decision making always a choice among alternatives? This leads on to the next assumption.

Attempts to fit simple minded models to decision or use them for decision have not been generally successful. Research focussed on cataloguing limitations, systematic errors, of "decision making" is not very useful without an overall view of the issues involved.

I have in mind here the notion of illusory correlation — people who attribute connections to events simply because they occur in close temporal juxtaposition. Another example would be the Law of Small Numbers, developed by Kahneman and Tversky. While these are of interest, they avoid many of the interesting issues in decision making. For instance, the concept of decision is central to a system technology which aspires to imporve performance. Thus, this general feeling translates into a nagging uncertainty over

approach. Can one improve decision without adequate description or theoretical basis? Not being inhibited by ignorance, the answer is clearly, yes.

Can we support or aid the decision making process?

Decision aiding can be defined as any technique or procedure that restructures the methods, techniques or procedures by which problems are recognised and resolved. The concept of decision support implies a focus upon the allocation of functions between men and machines - Divide and Conquer. However, this depends upon the assumptions and structure of the decision making framework from which it is designed.

The usual approach is to reduce the specific problem (eg. diagnosis of kidney failure) to a mathematical model (eg. Bayes Theorem) and assign some parameters to man and some parameters to machine. In a limited context this approach has been quite successful with specific problems . However, it is limited to "well defined" recurring problems and is sterile of suggestion for more general problems.

This goes back to the foundational difficulties, in that you cannot place this very limited data in a larger, practical context to provide a jumping off place. To clarify this we need to define a class of interesting problems.

Tactical decision making represents the class of problems of my concenn.Military decisions have a number of characteristics which non-uniquely distinguish them from decision making in general. (Table 2)

Well-defined "strategic" goals.
Fuzzy, multidimensional tactical goals.
Significant action alternatives.
Relatively high stakes
Inconclusive information
Limited time.
Highly structured institutional framework
Well established standard operating procedures and customs

Table 2. A class of Interesting Decisions

The headings in table 2 are fairly self-evident, and I will only comment on the last one. The problems are made more challenging in that they occur in a highly structured institutional framework. There are well established operating procedures and customs. Many things are done by routine, and are rigidly prescribed. The challenge is that anything that improves decision making must be incorporated into this framework.

The first step in meeting these problems is to have a description and a model of the decision process. Descriptions of decision are numerous. A common pitfall being the erection of a theoretical framework from the results of a single experiment. This pitfall is avoided through an absence of data. The intent is to structure, albeit loosely, the problem of decision in order to provide a perspective organising information, suggesting gaps/voids, and a basis for improvement.

Decision is considered as the application of a process (naturally, a decision process) by an individual to a task environment. One such process is outlined for convenience in linear stages, although the structure is actually quasi-hierarchical. (Figure 3)

1. *Problem Recognition/Detection*
   (a) *Assess problem urgency and importance*
   (b) *Classification of the problem as repetitive (an alternative can be chosen based on applying known rules) or non-repetitive (alternatives are unknown/rules for solution unknown).*
   (c) *Problem definition*
      (i) *specification of goals*
      (ii) *identification of goals*

2. *Planning/Information Acquisition*
   (a) *Determine whether more information is needed*
   (b) *Identify possible information sources*
   (c) *Determine whether or how much information is to be obtained (seek more information)*

3. *Problem Structuring*
   (a) *Definition of the decision space of the problem*
   (b) *Representation the the task - "cognitive map"*
   (c) *Choice of a decision criteria maximization, satisficing , etc.*
   (d) *Identification of task boundaries*

4. *Development of "Feasible" Alternatives (Plans/Hypothesis Generation)*
   (a) *Closely related to problem structuring, but convenient to consider separately.*
   (b) *Brainstorming etc.*
   (c) *A plan is an anticipated string of actions which increases the chances to attain a goal and with a minimum feasibility.*

5. *Exploration of Alternatives*
   (a) *Cognitive simulation*
   (b) *Future time perspective - a plan with a "minimum time schedule" for actions and goals.*
      (i) *time horizon*
      (ii) *description of uncertain factors*
      (iii) *description of consequences*

6. *Evaluation of Alternatives*
   (a) *utility*
   (b) *uncertainty*
   (c) *risk*

7. *Selection of an Alternative*
   (a) *future decisions necessitated or influenced*
   (b) *time required to implement*

8. *Initiation of Selected Alternative*

9. *Monitoring of Implementation*

10. *Evaluation of Consequences*

Table 3 A Decision Process

The process indexes a continuum beginning with problem recognition and ending at some later stage.

The number of subprocesses or stages is somewhat arbitrary and are located at somewhat arbitrary points along the continuum

The process is directional, proceeding from problem recognition through implementation and monitoring of consequences, folding back into problem recognition.

There are an infinite (large) number of potential sequences through the set of stages

Two (neglected) classes of variables, in addition to process, are individual and task

Choice among alternatives is only one stage of an interactive process and greater leverage may be obtained at other stages, eg. option generation or planning

A coherent basis for decision must include individual differences (sometimes called the last refuge of scoundrels). What parameters can /should be used to describe individually and culturally consistent "cognitive styles" and methods?

The task as defined by the decision maker, is an essential ingredient of decision . Note that few models of decision explicitly include task variables (exceptions include Conversation Theory and Probabilistic Functionalism/Lens Model).

A theory of decision requires a representation of memory, perception, etc.

The structure is not particularly unique: numerous similar models have been suggested (Vlek, Nickerson, Schrenk, etc). However, the perspective suggests several points of departure for improving our understanding of decision

In summary, there is a need for better conceptual frameworks and more adequate theory to guide research aimed at both understanding and improving decision . I have attempted to identify a number of threads or characteristics of decision which such frameworks and theories should account for.

Discussion

Pask: It might be interesting to debate for a moment how it is that we select between alternatives.

Sometimes it is obvious. For example, if I'm driving a motor-car and there is an obstruction in the road, I decide "to turn right or left", meaning, in fact, I select "right or left" because the steering wheel of a motor car will not turn to the left and to the right at the same moment.

But the great majority of selections are between processes or plans which are assigned values -- this is point 6 in the taxonomy, (point 7 is the selection). There are no obvious sets of alternatives unless they are highly contrived, as in a gambling casino, or given by, the environment, as in the motor car example. Usually it is the process that "contrives", and a definite singularity or bifurcation occurs. Surely process bifurcations are points at which selection is made.

What is your view about the generation of selective "alternatives" when they are not given, (and frequently they are not).

Johnson. I put in a stage called Exploration of Alternatives. I think I can convincingly argue that in many cases alternatives are not constructed, rather a course of action is constructed. I think this is the case which you are referring to.

Pask It is indeed.

Johnson. In many cases, some of which we've looked at recently, the problem of alternatives comes out very clearly. We've been interviewing decision makers on what they see as the pre-requisites and the difficulties of decision making. The most difficult stage is structuring the problem. They see no difficulty in choosing which course of action to follow, only in coming up with, and developing a course of action.

Atkin Does the structuring of the problem only involve the selection of alternatives? I think Gordon was saying that alternatives were not necessarily a partition of the possible events. It may be a cover, so that events are highly connected, and that makes it much more difficult to find tracks through this weird space.

M'Pherson. Is there any difference in kind between the designer looking for feasible alternatives and the decision maker looking for feasible alternatives?
Is the designer of an engineering device "partitioning possible future events", or doing something completely different? Look at it from these two approaches. The engineer is usually meant to be dealing with something very concrete, specific and mundane. The decision maker with something that is fuzzy and woolly that distinction might clarify the issues; except that as an engineer who knows a bit about design, I really think a designer has as big a fuzzy problem as the decision maker.

Helme: Here I wonder if the contrast isn't in the real pressure of the time vector.

Pask I think design is a special case of decision. We have studied designers, and they differ from decision makers in having a brief which is rather well defined. In the general case you have policy decisions, whereas the brief of the designer is to manufacture an ash tray, or a piece of electronic circuitry, or a central heating system, and so on. Now designers operate in different modes, and they make decisions all the while. One can call the bifurcation points decisions. I would tend to call them choices or selections. Both designers and policy makers can be allowed a fairly leisurely approach. The designer decides in a domain where some kind of objective exists, whereas the decision maker may, in a very real sense, not know what it is. He is a multi-purpose creature. He may float between various task domains. Decision is the whole lot. Everything on the process chart and probably more.

Nicolis I would suggest a more modest approach. Decision making can be considered as aninevitable and spontaneous by-product of a conversation going on between the individual and the environment. Successive, distinct bifurcations arise from the instability of the existing steady states whenever some parameter exceeds its critical point. Then you have a real catastrophe, a real turnover of behavioural modes. In this respect decision making is an unpredictable process. You cannot hold a list of possible courses of action unless you are indulging in comfortable and trivial armchair thinking. In real life, in real time, decision comes as a suprise.

Johnson: Not always. Simply recognising and structuring the problem may lead to an action which in retrospect, you call decision.

Hogarth: This brings up a point over which I would disagree - decisions made by habit. I find the dichotomy of overt and covert processes problematic. Decision can creep up on you and all of a sudden you've made your decision without realizing it, you've invoked a past habit. Driving a car is habit and only comes unstuck when you go from England to France and suddenly find yourself on the wrong side of the road. This is important, because a lot of decisions seem to appear out of the blue.

Pask I think that you've stashed away the habit. Decision perhaps ought not to be regarded as the decision of the driver, but the decisions made when he learned to drive, using the descriptions, heuristics and algorithms given by the driving instructor. And this is especially important in the context of training.

Nicolis Can we come to the question of commitment, as there are important cases where this concept is useful. Consider a class of decisions that result from a resolution of some inner conflict. Say the inner conflict between remaining what you are, the homeostatic tendency, and opening yourself to the environment. There are ways to optimize this conflict and its resolution, as a behavioural act, that can be considered a factor in decision making.

Unless you are endowed with the proper amount of guilt and remorse you are not a good decision maker. Consider two opposites to make the point clear. A man who has no inner conflicts does not feel guilt or remorse over having no self-imposed rules, or failing to actualise hidden potentialities. He acts impulsively, like a real psychopath. He is a very bad, very irrational decision maker. At the other end of the spectrum, a man who is full of guilt and unable to resolve it falls into behavioural paralysis. No decision at all, good or bad. Therefore, I take the rather radical view that young people should be trained in the traditional way, inducing the optimum amount of guilt and remorse when they fail to recognise self-imposed rules or actualize hidden potentialities. Without this, you drift hopelessly towards either impulsive, psychopathic behaviour or behavioural paralysis.

Pask: I agree with that, but what you call guilt and remorse, I would call responsibility. The point is that responsibility is a quantifiable commodity. This relates to work by Ron Atkin and by Elliot Jacques.

Gaines I agree with most of that, but I was going to construe it as anxiety. It will actually help push a child up a level of abstraction because it induces a number of self-observational skills.

M'Pherson We are talking about decision making at different levels and in different contexts at the same time. It will be useful to sort these out over the conference. I can't see much remorse or guilt effecting decision makers determining the best nuclear policy within the energy program — perhaps I should. Are there distinct, different characters of decision making — say between the personal behavioural characteristics of individuals and high level group decisions on abstract policy.

Johnson The resolution of conflict, the problem of guilt and remorse, is part of a process beginning with problem recognition. You are really talking about the genesis of the problem, which may be in intra- or inter-individual conflict. Habit is excluded if its an autonomous process, included if there is a bifurcation, at which point there is a conflict, a problem to be structured. Conflict can be intrinsic or extrinsic, but usually, as long as things are smooth, you don't disturb them.

Braten A prevalent assumption here is that there are a set of law-like regularities which apply at least to the referent domain of the decision making domain, and which make it possible to recognise and structure problems, and to search for action alternatives. It seems to me that the viewpoint is changed if one assumes that one operates in a number of possible different worlds. Each of these may show unique regularities, but these cannot be generalised, and conflicting tendencies may even be found. The next step would be to form images of these worlds, of their conflicting tendencies, and so on to the next level. These levels can be illustrated by substituting the word "world-image" for "action" in your chart.
Also, when one speaks about decision making, it is as if decision makers move through the world, and my feelings are usually that the world moves through decision makers.

Johnson. The only distinction I would make is that problem recognition occurs in a world. This may not have very much relationship with somebody else's world image, but rather thats an image on which .e process is operating.

Thematic Summary

Foundational: decision making extrinsically defined. "If you don't know what to call a piece of behaviour, you call it decision making".

Decision as a choice among alternatives; origin, nature, and limits of alternatives.

Role and limits of "simple" models.

Well-defined recurrent problems vs. ill-defined complex problems.

Decision process taxonomy.

Generality/unpredictability/spontaneity of decision

Practical/atheoretical decision making and decision aiding

The importance of the decision maker in the process

Psychological concommitants (habit, guilt, responsibility, anxiety) .

Decision and design.

Section 2

Observable Components of the Decision Making Process and a revised theoretical position

Gordon Pask

Overview

Dr Pask's paper, in Volume II, describes a complex individual or team decision system employed for studies of training and general performance under more or less severe conditions of overload. The system is designed on the basis of Conversation Theory . The scenario is an allegory of "space", each subject (one or two) acts as a mercenary in charge of two "Spacecraft" which can be manoeuvred and used to obtain information. Hence, subjects are required to maintain more than one focus of attention if they are to succeed in the task.

The task itself is based upon a universe containing 4 fixed starbases which maintain their energetic economy by trade, namely, an activity in which barges of goods are exchanged along trade routes, which connect the starbases in an initially torroidal universe. The mercenaries are required only because space is periodically invaded by "Klingons", which leach the energy in starbases and interrupt the trading barges.

Mercenaries are employed (and their spacecraft are provided with the energy needed to move, demolish invaders, or obtain information), because the number and distribution of "Klingons" would otherwise disrupt the starbase economy.

A subject's conceptual operations are exteriorised as behaviours:

(a) By instructing the commanded spacecraft (to move, demolish Klingons, obtain information of various kinds).

(b) By periodic interrogation using a behaviour-contingent question-set, which is generated automatically during the conduct of an experimental session.

(c) By building an entailment mesh (from which all possible plans can be derived) to represent a subject's "world view" of for example, the spacecraft and their tactics, starbases and trading routes.

The behaviour could be devoid of cooperative interaction apart from a deliberately built in discontinuity or process bifurcation. If the balance of energy expended in eliminating Klingons exceeds a critical value, then the torroidal space is cut, by "cracks", into cylinders or rectangles (disturbing both trading routes and the motion of the spacecraft). Crack repair is possible, but only if there is cooperative interaction.

Amongst the studies performed in the space (team decision) system, some have examined the relation between the scores on pretests for conceptual style, and the process of decision making as manifest in the recorded behaviours.

The system description of the paper in Vol II is given in greater detail, together with various kinds of data, in Final Scientific Report No DAERO 76-G-069.

Commentary:

Pask: In his book on the philosophy of space and time, Hans Reichenbach notes a comment by Kurt Lewin about "genidentity": Mr A and Mr B are different, both today and tomorrow, but Mr A today and Mr A tomorrow are regarded as the same, as are Mr B today and Mr B tomorrow. In embarking upon a serious attempt to look at team decision making - perhaps the most difficult of all socio-mechánico-psychological problems - it seemed that the concept of a team made no sense whatsoever if we maintained genidentity. In other words, the entity said to be "deciding" must be distributed over people, places, and systems. So "where does decision making occur?" The answer to this question appears to be "in a coherent organisation". Obviously, a "team" or any aggregate of people credited with decision making is an exemplar; so is "one individual", and so, it will be maintained, is "an organisation of people and various other-than-biological tools"; here, computers or similar devices. Invoking non-acceptance of genidentity, it is possible to countenance decision making as a quality of the entire decision system.

The next philosophical issue of team decision was the "game" paradigm. Here there are some presuppositions which are suspect, and positively counter-productive in the context of decision making by teams and (possibly) individuals.

In most game theory (also, "decision theory") it is assumed that players are, as it were, separated into "black boxes", or placed on opposite sides of a screen. They are identified, but do not interact, and they maintain their identity throughout the game. Another supposition is that the players are "competing" or "cooperating", or possibly, something in between. These words are used to designate the general forms of game theory determined by looking at the payoff matrix (or by the payoff matrix if it is constructed as a result of ruminating upon outcomes and saying "yes, that is an outcome I would like", or "having a reasonable preference", which is assumed to be invariant).

This is all right in a short game, or a casino, where everybody has a normative contract to win as often as possible. Can competition and cooperation be dissected out from the process of decision in real life? The answer is "no". Even the most

cooperative of players are still anxious to gain kudos, even the most avaricious of players will cooperate, under stress, to survive. I am loath to regard a theory in which a situation is called a priori "competitive" or "cooperative" as a general paradigm.

Whenever the players are pushed outside what amounts to a casino or a dice throwing environment, they act in a complex, many faceted, manner and do, willy nilly, try to interact. This point has been made recently, by Robinson, Howard and Rappaport, in a dialogue concerning "Prisoner's Dilemma".

The next philosophical point refers to the objectives of decision making; usually assumed to be "goals". However, in classical Decision Theory, "goal" has several meanings. One is a description, a subset in a descriptive space, containing points that represent desirable states of the system. Another, is the notion of an ongoing action, and an intention to adopt this action. This is closer to the current interpretation; decision makers are first impelled to adopt a "world view" in which descriptions and intentions may be determined. The world views taken by the person or team making the decision are neither descriptions, nor action specifications, nor intentions. They are "representations", and have some or all these qualities, depending on whether they are used descriptively or prescriptively.

Given the representation of a "world view", it is possible to question the existence of alternatives and the distinction between outcome and actions. In a casino, for example, outcomes are well demarcated. Action leads to an outcome by a deliberately contrived mechanical process. But in real life we choose, or prefer, ongoing actions and ascribe them value.

That is so in the great majority of situations, including, for example, motor car driving. We do not actually choose between turning to left and right, but survey a series of possibilities which may, according to the constraints, be allowed to run in parallel or only to run serially. Turning the driving wheel is a

serial example, communicating with other drivers (by implicit or explicit signals), and most manoeuvring, are parallel cases. To place a value upon actions is a different matter (as Brian Gaines points out) from placing a value on outcomes.

Descriptions of possible actions may be envisaged, ad libitum; but action, or a process giving rise to an action, will polarise and give directionality to the representation, yielding different plans and behaviours depending upon the point of view (the perspective) that is adopted. The usual notion, that alternatives (game strategies, game choice sets) are given, a priori, is not at all typical. The fact is that any process gives rise to singularities, or bifurcations, where only some of the actions may take place. With John Nicolis, it is possible to distinguish between "Inessential Bifurcations" where information transfer is needed because "at least one" of the "only some" actions must take place and "Essential Bifurcations" where none of the actions is compatible with the existing descriptive framework.

Under e circumstances, there is a catastrophic boundary, a catastrophe in the sense of Rene Thom or Chris Zeeman, in the system equations. It is now necessary to predicate or compute a distinction, and to augment the description.

Both kinds of bifurcations may be induced by obstructions in the environment, or by constraints upon the process of conceptualisation; for, according to the answer provided for "where are decisions made?", decision making is distributed over the entire decision system. The system environment boundary, on which the considerations of fixed alternatives-outcomes are based, is fictional; obviously so, in team decision making. There are inessential bifurcations which may be regarded as choices (throws of dice are specially arranged instances), and essential bifurcations that lead to a revision of the "world view" representation which is a framework for decision making.

The distinguishing feature of Essential Bifurcation is that a point of view or perspective - necessarily adopted when a representation

is employed prescriptively, optionally, if it is employed descriptively - proves inadequate. Some other perspective is adopted and the representation is modified; possibly enhanced in the light of further data. At least two perspectives are thereby juxtaposed - there are potentially conflicting points of view. The resolution of the (potential) conflict is the construction or creation of an analogy between universes that are distinguished by the computed predicate. The juxtaposed perspectives may be entertained by different members of a team or be simultaneously entertained by an individual. In either case, I maintain that this phenomenon is ubiquitous, and that, if resolution occurs, it corresponds to Pierce's abduction (of an hypothesis, or plan, or design). This usually brings in its train other aspects of what, in the early Renaissance, was known as induction, though deductive transformation is also possible.

If you accept these points as valid, then it seems that the similarity heuristic, noted by Kahneman and Tversky and discussed at the first conference, is universal. Insofar as Inessential Bifurcations (choices) are not predictions of the unpredictable, the decision maker recognises similarities between this choice situation and others (probabilistic inference being a very special, seldom encountered, case). If essential bifurcations are resolved, then analogies are constructed or created, but any analogy is based upon a similarity between distinguished perspectives, or universes.

The superficially reasonable question, "When is a decision made" is generally unanswerable (or is even nonsensical) in the context of a system having an evolving world view representation. Here it is especially important to examine the correspondence between Ron Atkin's contribution, John Nicolis's, and my own. Very crudely, Atkin's "backcloth" is my "representation". Changes induced in "traffic" upon a backcloth are decisions induced by Inessential Bifurcations, and changes in the backcloth are decisions induced by Essential Bifurcations. Given that much, Atkin provides very good reasons for saying the question, "When are decisions made?" must, in general, be unanswerable, and, using the usual "point, interval" topology, must remain so.

As the last philosophical comment, it is very questionable whether or not people are able to estimate preferences independently of likelihood, degrees of belief, or probabilities. Since the assumption that they are able to do so (at least that "rational man" or "economic man" is gifted in this way), constitutes a kingpin of game and decision theory, the matter is significant. Surely the possibility of independent assessment exists when the conditions are propitious - alternatives and outcomes not only exist but are agreed to have reality. These conditions seldom apply in a complex decision system, as noted in the context of value (coexisting with process) and alternatives (generated by a process). If these quite rigid conditions do not apply, there is no reason to place much faith in the independence of likelihood and value; or, in fact, much faith in game theory.

I do not believe that complex decision systems are game-like; *nor, in the formal sense, are many* business games and simulations. But "if they are not game-like, then what are complex decision systems like?" My reply is "an allegory, a story, a dynamic metaphor designating a reality in which A today is strictly analogous to A tomorrow, B today is strictly analogous to B tomorrow, and in which A may or may not be analogous to B". <u>But</u>, whatever else, A today is not the <u>same</u> as A tomorrow, B today is not the <u>same</u> as B tomorrow.

Those are the philosophical points of chief concern; some theoretical matters are still worth highlighting, but at this juncture, I would like to turn to the discussion, and examine the project reported in the paper, its methodology, and some empirical findings.

Discussion

Pask. The team decision system which you can see and use at the laboratory is an experimental device but the project brief is fairly pragmatic; the system is used to investigate man/machine/team-and-individual decision making, with a view to determining (testing, predicting) decision style. It exteriorises the usually hidden conceptual operations involved in decision making, and is also used as a possible training ground for decision makers. The research is set in the context of complex decision making, in command, control and communication systems, both for individuals and teams.

The team decision system, as it currently exists, has the following component subsystems.

(1) A mission, of which the decision task is an allegory, which is based upon an environment containing objects (such as vehicles, marauders, entities to be protected).

(2) Performance of the mission, by individuals or teams, able to exert control over some, or all, of the objects and to fulfil the mission: failing which Essential Bifurcations must occur; they may occur, otherwise.

(3) A planning subsystem in which individuals or teams represent their world views externally as entailment meshes, derive and possibly execute plans.

(4) A so called "interrogation" subsystem which questions the individuals or teams about their current state of knowledge by asking questions which depend upon the situations encountered in their mission, ie. "behaviour dependent" interrogation.

(5) An executive which currently logs the behaviours and situations but may also act, independently, on the basis of existing representations and plans.

(6) For teams, a communication subsystem.

The system is the end product of a research project and is believed to be the least complicated system that allows for a proper externalisation of the decision process.

The brief included a requirement to relate score profiles on previously established tests for learning and conceptual style - description building ability (comprehension learning), procedure building ability (operation learning) and ability to extrapolate (versatility) - to decision style and overall performance.

It turned out that a system able to exteriorise complex decision making also has an operational potential. It can certainly be used, by human beings, as a decision aid, and it may, at certain points in the process, over-ride the human beings and make decisions on their behalf by using the representations and plans they have previously revealed to it. The points in question are those points at which, in standard decision theory, the manager must do something and can (within that standard theory) do no better than toss a biassed coin, or consult the entrails of a fowl; the last resort of an oracular forecaster.

Robinson. Could you tell us how the decision system evolved?

Pask. The research project started out, conventionally enough, by looking at military and business simulations, and later, at TV games. The computer game called "space war" was used in the laboratory. It is trivial and only one feature survives in the current system, a marauder called a Klingon. In "space war" there is one spacecraft which has two kinds of weapon. It has a universe perturbed by a random generator emitting Klingons which leach energy out of spacecraft and other objects. The subject has to eliminate these things. Looking at records we found, like many other researchers,

(a) that people learn the task but show no evidence of learning to decide, or learning about the underlying principles. This result was noted at the previous conference by Donald Broadbent in the context of business simulations.

(b) that people learn reliably to have (at least) bimodally distributed response latencies. Diagram 1 shows typical latency data. Brian Lewis and I obtained similar results, many years ago, for a perceptual motor skill.

One peak represents "fiddling around with details" and the other peak "control by global overview".

The next step was to program a more complex game, with two ships, the random generator and much the same scenario. Again there is no noticeable "learning to decide" but a great deal of learning the task, manipulating a vehicle, how to deploy vehicles and a tendency to delegate and distribute control or to use one vehicle at once. The task had signs of being a candidate decision making task. One of the most marked effects is a fixity or rigidity which developed during the course of performance. Diagram 2 shows some data, relating crude indices of decision making to pretest scores for learning style.

A substudy was carried out to obtain the result rather dogmatically asserted in Fig 19 of the paper; that doubt has many dimensions (doubt about descriptions, methods, and outcomes, are some of them). These forms of doubt (conversely, of certainty) change in a regular and predictable way as a concept is learned and stabilised.

The next part of the research project introduced teams of two people playing the neutral role of mercenaries, each person controlling two spacecraft and able to delegate control and manoeuvre craft using restricted tactics. The space environment is deterministic (Starbase Trading) and Klingons are generated by a learnable-in-principle (though too complex to be fully learnable-in-practice), deterministic process. The topology of the environment can be changed (as a result of cracks and holes that impede movement and trade), if energy is dissipated in the overzealous elimination of Klingons. Moreover, if the cracks or holes are not repaired, some part of the system runs into an emergency (an essential bifurcation). The system is an allegory in which it is possible to observe a context dependent fluctuation of competitive and cooperative behaviour.

The system described in the paper was obtained by integrating into this allegory (with the mission of "being a mercenary") a behaviour-dependent interrogation (as one measuring instrument), and partially integrating the construction and manipulation of planning-entailment meshes (as the other).

van de Veer You have said that the machine is able to exteriorize concepts and plans, even agreements in the case of teams, and that it is also able to take control in the event of an emergency. These are very important points. Could you say a few words on how this is done?

Pask The THOUGHTSTICKER system was originally built as a means of exteriorizing theories and expositions of educational topics. The rules of operation form a language which guarantees coherence, and they are fairly uncontentious. Coherency or agreement is obtained through the machine. The manipulations, including overgeneralization of theories, are done by the machine.

Nicolis What are the criteria for the selection of the best model when the machine takes over control?

Pask There are two points here. Firstly, the total research would be successful if the executive machine, which does not yet operate in the automatic mode, were effective. Ultimately, I would like the machine to be mistaken for a participant in a decision system. All the material and theoretical prerequisites for the automatic mode exist, but the machinery is not yet coupled together. That is a very large job. You have to have a lot of little machines running asynchronously and able to communicate, rather than the existing LISP -- and hence serial -- organization of THOUGHTSTICKER.

The second point is that there is no such thing as the best model or plan in the abstract -- hence there is no single criterion in the classical sense.

The theoretical framework of the research is "conversation theory" as spelled out, ad nauseum, in the paper. Apart from a change of idiom this framework and Stein Braten's theoretical framework are isomorphic. Decision is seen as a kind of conversation.

Of all other "decision oriented" theories, "Decision analysis" is probably the most liberal discipline, and is close enough to the conversation theoretic stance of Stein Braten and I, to warrant comparison. Decision analysis is essentially a conversational technique, whereby it is possible to arrive at a consensual or, as I prefer to call it, a coherence agreement between the decision analysts and their one or more clients. In a classical decision analysis, as set out by Raiffa, for example, this agreement yields a tree structured model. An

elaborate block diagram or functional diagram is whittled down to one, or several, alternative tree structures, so that it is possible to assign choice probabilities to the branch points of the tree. The outcome points may be assigned utilities, probabilities and the compounded expectations.

Up to this stage, our method is similar to an "on line" form of decision analysis, but at this stage there is a divergence. The great majority of real world situations are not tree-like in character. Hence, probability measures do not, in general, apply to them. Raiffa, for example, talks about a "tree" becoming a "bushy mess", with an infinite collection of open options, representing, in fact, an inherently cyclic or circular organisation. The conversation theoretic method avoids this problem by not insisting upon tree structures in the first place.

The conversation theoretic scheme admits the concurrency of planned actions (in contrast to the serial and simulated-parallel actions, permitted by a tree-structure). So, for example, several vehicles can be manoeuvred, allowed to communicate, sometimes independently, and sometimes with dependency or interaction. Conflict as well as cooperation is possible, though there are procedures for conflict resolution.

The main disadvantage of this representation of plans and actions is that, unlike a tree structure, probabilistic measures of expectation and likelihood are inapplicable.

Of course, other measures of the aleatory, the uncertain, and the ambiguous, are available; a couple of candidates are the likelihood indices of Ron Atkin's paper and Lofti Zadeh's possibilistic measures (where "possibility" is used, as it is by Brian Gaines, as a designator of "coherence" comparable to Rescher's). Either scheme calls for a fundamental definition of "hard data" underlying the "likely" or "possible" and conversation theory furnishes this definition as a "stable concept" of some real, or imagined, object or entity. Stable concepts (described in the paper), are comparable, for example, to Bartlett's schemata and to Wertheimer's productive as well as reproductive systems of thought, and invention. They are related to each other, and exteriorised, in a so-called "planning entailment mesh" which is the canonical representation of a decision makers "world view". Under different perspectives (themselves objects or entities) the entailment mesh yields different plans, compatible with the perspective and the "world view" which, if executed, resolve doubt.

Hogarth: Have you found any relation between these exteriorised plans and criteria for the evaluation of decision making?

Pask Plans are evaluated for effectiveness if an emergency occurs and the participants are unable to carry out their mission. Suppose that the individual commanders, jointly or not, have expressed and updated their world-view representations as entailment meshes, and that they have assigned tactics to their own spacecraft.

If there is time, the commanders can be presented with plans, derived and displayed from their planning entailment mesh under the perspective of the object in emergency. In an ideal, not yet realised, system, the mesh would be updated with interrogation data. The commanders may express, at least a preference for one or other plan. As the time that is available decreases some plan or plans must be executed, driving the spacecraft automatically, and, as the last resort, if nothing is done, the system (containing the previously obtained plans and interrogation data), must act on its own.

If we allow this, then we can judge the participants as decision makers according to the following criteria. How complex a mesh representation can they output? How complex a role, or actor, representation can they make? How far ahead can they look? In this context -- on the assumption that interrogation and planning has taken place -- another measure of performance of the team or individual is the converse of emergency action: the extent to which emergencies are anticipated and avoided.

A possible overall answer to the question of assessment is as follows.

Good decision makers, in this context, are precisely those people who have a long "time span" in Eliott Jacques sense. Eliott Jacques has studied the "time spans" of jobs and has observed the assignment of people to occupations that suit and do not suit them; according to whether the "time span" of the occupation matches their own responsibility. His methods do not yield a direct index of personal "time spans" (or a set of indices that are reasonably called "responsibility"). It looks as though the decision system, augmented by the "event times" noted in Ron Atkin's paper, does provide such an index, the "event times" being

estimated through a structural analysis of the current planning mesh representation, updated and refined by interrogation data.

It is proposed to quantify responsibility in this way. As a conjecture, responsibility is as good an index as any of decision performance. It should be possible to fit particular missions to particular mixes of conceptual style, at a given level of responsibility.

Thematic Summary

Foundational: "genidentity", perspectives, individuals, and teams: decision identified as distributed over systems. Decision theory as based on interacting perspectives. The evaluation of decision making - the roles of representational complexity and responsibility.

Limitations of decision regarded as a choice among alternatives; the artificiality of "alternatives". Well defined recurrent problems vs. ill-defined complex problems; inessential and essential bifurcations. The creation of alternatives: the roles of analogy and abduction.

Hard data and common agreement.

The nature of problem complexity: time compression and "emergency".

The role of computers: parallel and serial consequence generation, conflict resolution, and automatic decision taking.

Section 3

Decision making as an Event-search: traffic on a Multidimensional structure

R H Atkin

Overview

Dr Atkin's paper in Volume II is a comprehensive mathematical (topological) essay on relations and their often complex implicit structure. The relations in question hold between observations (hard data) or events described at some grain of scrutiny. By means of a technique called Q analysis, it is possible to speak of a relatively invariant "backcloth" or frame of reference, S(N), at some grain (N), where N is arbitrary but N + 1, N + 2, ... are specifiable relative to the chosen origin. On the "backcloth" S(N) there is usually a traffic ie, a flux of activity, which is constrained by the existing relations.

If all of a structure S(N), S(N+1), ... is regarded as a composite hierarchy, at level H, then it is possible, also , to examine the perspectives of (say) planners, or strategists, in contrast to observers or tacticians, as occupying levels H + 1, H + 2, ... and so on.

Amongst other things, Dr Atkin develops an external algebra for manipulating the "complexes" of "simplices" which describe the inherent structure of the prevailing relations and a means of imposing (Direct sum) graded patterns on the traffic over these structures. Such complexes of simplices may be represented geometrically in a Euclidean space of proper (and often very high) dimension.

One salient feature of the model (or language) of Q analysis is that graded patterns can be specified for temporal sequence, to give dimensionalities of temporal process appropriate to the social and behavioural sciences, where the limiting cases of Newtonian and relativistic order are manifestly inappropriate.

Another salient feature is that likelihood or expectation of events turn out to be a higher-than-ordinary dimension, except in the limiting cases of (classical) "decision theory" or simple "probabilistic selection".

Commentary

Atkin : I have been working at the methodology of Q Analysis for the past few years, and trying to apply it in various places. My paper was a very concise summary of the stuff which normally occupies a lot more space. Its relevance to decision making is something I've tried to find out in writing the paper. I have been told more than once that it's highly relevant, but I'm no expert on the whole business of decision making. I'm also quite ignorant of psychology. In fact, the only thing I really know anything about is Q-Analysis. And in that respect I have a slight edge on the rest of you, so you must forgive me if I talk about it in its own right.

The work is basically an analysis of the relations between finite sets. It has led me to the belief that the methodology of science is about finite sets. Hard data in the history of science seems to me nothing more than the identification of set membership.

This goes back to Galileo.. I believe the contribution Galileo made to the world had nothing to do with the earth or the sun. It was his experiments in rolling a ball down an inclined plane, and his deduction that the distance travelled was proportional to the square of time. In doing this he set on an experimental basis the business of identification of set membership. One set of so-called geometrical points, the other set of hard moments of time. This was the experimental verification of work which had been going on for the previous 200 years. He developed a hard language, largely geometrical, but mathematical because it was based on set membership.

It was unfortunate that the Roman Catholic Church didn't understand how serious this was. He did it largely after he'd been put under house arrest, after all the fuss about the sun and the earth. They created willy nilly an atmosphere in which he could do these experiments, and start the science really moving. I think this was a kind of divine comment on the whole situation.

Now social science, which I might loosely call soft science, is not established on the basis of set membership data, and therefore not susceptible to severe mathematical discipline. On the other hand the hard sciences, physics, engineering, chemistry, are so established.

In the laboratory nowadays the scientist has handed over to the instruments the role of deciding set membership. The galvanometer tells the physicist what is or is not a member of the set of electrical charges. The physicist does not any more have to do it himself, but there was a time when he had to. The great Cavendish, who started the Cavendish laboratory, had a reputation for being able to do this. He used to grasp the leads of a Leydon Jar and quiver. Then he would say "two and a half Leydon Jars" or whatever it was. He had a reputation for being pretty accurate. But even so, it wasn't hard data. Now the world of engineering and physics is hard. Its "out there", run by instruments, and it looks pretty easy. But deep down there is this terrible problem of what is a set membership.

If we come to the soft sciences, psychology, sociology, medicine, maybe economics, we find ourselves saying "where is the hard data?", "how do we find it?". When the sets are not well-defined, we are stumbling in a soft world in which its very difficult to establish anything we call science. Getting the sets well defined means they have to be agreed among the participants. I find Gordon's remarks about sharp data exactly a prescription for telling you how to find such sets, or define them and make them sharp, so you can start this whole process.

If you have data which can provide you with sets and relations between sets, then this gives you what I call the backcloth. The physicist's backcloth is his three dimensional space -- which, I might add, is never experimentally observed, except rather casually. When soft science finds a comparable backcloth, we've hopefully got to be able to have, what I call traffic, moving around. Relations between finite sets naturally give rise to the notion of simplicial complexes, genuinely multi-dimensional complexes — from a pure mathematical point of view these are interpretations of relations between finite sets. There is a theorem, well-known in mathematical circles, that every simplicial complex can be represented in a suitable Euclidian space as a complex polyhedron.

Here is a very simple simplex. A two-dimensional or two-simplex. It merely illustrates that the set is more than the sum of its parts. If you take the scheme of colour combination that was first worked out by Maxwell back in the 19th century, and is the basis of colour TV these days, then you have three basic hues — red,green, and blue.

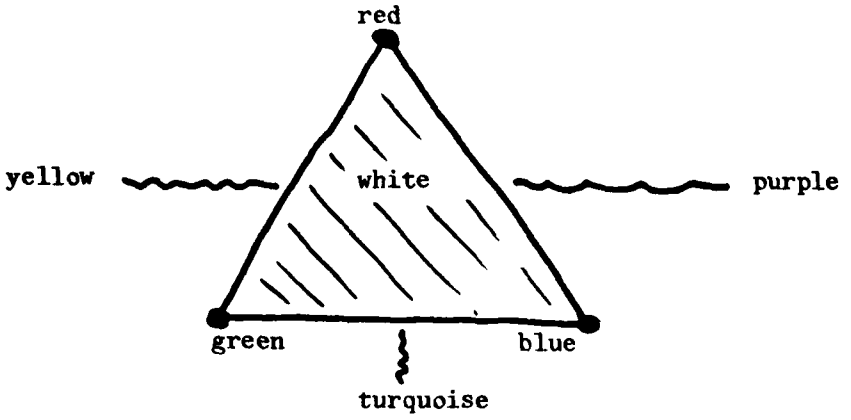


FIG. 1.

Red, green, and blue are the vertices. From this you can construct a two dimensional simplex, the triad, and its representation in two dimensions, which is the triangle. The shading means that if you see red, green, and blue simultaneously, you interpret it as white. The edges, one-dimensional things, are interpreted as other colours — yellow, purple, and turquoise. This is a very simple illustration of our experience of a two-dimensional simplex.

Now, if you had one-dimensional vision, you would not be able to observe this world of two-dimensional events. All you could see would be the faces. You wouldn't be able to distinguish between them, and would start using words like "the chance of it being yellow" or"the chance of it being purple". If you had zero dimensional vision, and someone showed you white, you'd say "well, its blue" or "its red" or "its green".

I suspect the notion of probability or chance arises in the context in which we have a hard backcloth -- which might be a very elaborate structure of many dimensions — but we are only capable of observing sub-spaces of the one we are in. But if, in the illustration, we are two-dimensional people, the notion of probability does not arise. If somebody shows us the triangle, we say "there's not doubt about it. Its white."

And the same thing goes for throwing a die. A six faced die is really a five simplex in this approach. It has six vertices and so is a five dimensional thing. In order to see the die as it is with six vertices you need a five dimensional method of observation. Throwing a die is really a zero dimensional traffic, a zero dimensional observation on the structure.

The notions of probability, chance, expectation, and the like are often appealed to when we are talking about whats going to happen because we are making a decision. But these notions only arise when we are talking of a high dimensional world, and our methods of observation are relatively low dimensional. I raise this point because, perhaps to get a real view of what the worlds about, we ought to raise the dimensionality of our observational techniques until we find that probability has been removed, we are back to certainty, and we see why.

I will end with the provocative remark that talk of probability and chance is just encouraging us to deceive ourselves.

Discussion

Gaines But if I move up in dimensionality, and just see a die, then I can't say anything but that it is a die! It is the low order traffic I'm actually interested in.

Atkin Yes, if you can see the whole structure, you can see all its faces. I would like to think that you can appreciate, not only white, but all the other colours that the structure contains. When you see the die, you would in some sense see all the sides at once, but you would also be quite free to see any subset that you like. Otherwise you would be missing some of the components of the thing.

Gaines But I want to predict what happens when its thrown.

Atkin. When you ask that question you have to identify what throwing is. There would be no uncertainty if the die were thrown by a machine of known slope at a known angle and velocity and so on. Human throws may deliberately, just for fun, be a zero dimensional traffic on this thing — and so you use standard probability numbers. But putting uncertainty in is the opposite of what you do in science. Suppose the thing you are talking about isn't just one simplex, but a whole lot of them joined together. Then you have the problem, not just of noticing all the vertices, but noticing whether the thing falls into two distinct pieces or not. The problem with the method of observation is that you can only move freely from one edge to another if they are suitably connected. If they are not suitably connected, you are on this piece and can't go to the other, then you have some fascinating problems.

Gaines I think you're saying that if you start with the assumption of probabilities, you may not be doing the right thing — not that if you end up using probabilities you've gone wrong somewhere.

Atkin. Yes. I think thats a better formulation. You ought to get the dimensionality of your traffic of observation up to the maximum dimension found in the complex.

Lewis Is raising the dimensionality of observation the primary purpose of the methodology?

Atkin No. Its an aside on the grading of traffic on a structure, and what it might mean in the context of probability ideas. Hopefully, it might provide the start for the discovery of some kinematics in the social science. The physical sciences, back in the Merton School at Oxford in 1350, established the need to distinguish between kinematics and kinetics. The Aristotelian confusion in kinetics was that they wanted to describe everything at once, to get at the causes rather than the description. Whereas if you want to describe the motion of projectiles, you need a kinematics. A very popular formulation of Aristotelian kinetics back in the 1300's was to say "the motion of a body is the realization of a body's potential". A soft explanation of its motion was that it was trying to get somewhere.

A comparable statement is found today in education. We say education of a body is the realization of a body's potential. This is just as soft. Where do we find an explanation, a kinematics, which could give us an explanation of education, as we have an explanation of motion in physics. I believe it is legitimate to want to achieve this.

Nicolis To put this in the context of decision making, which can take place at many hierarchical levels, is it possible to observe a decision maker simultaneously at more than one hierarchical level? Is it possible to follow statements and metastatements, language and metalanguage, without limit, and without falling into the trap of a double bind ?

Atkin I would have thought so, because in the hierarchical structure you were talking about the language would already be defined in a mathematical way. The relations between the various data sets would be hierarchically organized by cover sets and mathematical relations. If traffic can move vertically on this structure, then presumably it could be observational traffic.

M'Pherson One of the problems that affects people working on decision theory is trying to catch the attention of the people who make the decision. In your very interesting paper you talk about regional planning problems. This is about clarifying problems, and aids to modelling. Could you talk briefly about these studies?

Atkin I've dutifully kept many planners informed, and the interest has varied. A Chief Planning Officer in Norfolk wrote back saying he'd put my report in the waste paper basket, and if I sent him any more he'd do the same with them. As far as he could see it was "of no practical value, and also wrong." (laughter). I felt I was getting through to the man.

I did an analysis of visual connectivity for a conservation area in Southend-on-Sea at the invitation of the Planning Committee. On the seafront there two big, high-rise blocks of flats had been put up 10 - 15 years ago. Along the rest of the seafront were a lot of rather attractive, late Victorian/Edwardian properties. A planning application had been put in for a third high-rise block. The argument from the developers was that, since two high-rise blocks were up, there could be no theoretical objection to a third. The planners didn't like it, but had no weapons to argue against it.

The visual connectivity study meant that, at a set level, we were talking about the actual shapes of doors and windows, wrought iron workings, and that sort of thing. Another idea involved is that of eccentricity. This means that some buildings stand out from their neighbours as less connected to them. Eccentricity is measured by a simple number.

I showed quite easily by the analysis that the two high-rise blocks had non-zero, positively numbered eccentricity. Most of the other buildings had low eccentricity — they were highly connected to their neighbours. If you put in a third high rise block it turned everything over. The high-rise blocks became zero, and the other buildings began to have high eccentricity. A flipover situation was created. From now on the normal thing would be high rise blocks, whereas before it was not.

This finding was compatible with common sense, but it was nice that this approach brought it out. Similar things have been done in Lavenham,Suffolk, and Ely in Cambridgeshire.

A surprising, but somewhat depressing study, was on the political organization of our own University of Essex, back in 73/74. We were in the throes of much heady discussion about student representation and so on. Many members of staff had already intuitively decided that participation in committees was not a serious kind of decision making. Many of our students were put onto these committees. After about 12 months they just stopped going, and complained that they still had no power. I was disappointed to find that was a genuine conclusion.

Participation on committees constituted noise on the structure. It was traffic that had to keep on moving round great big holes in the structure, and decisions could not be made. This is a common organizational disease, but at least it was possible to put it quantitatively.

Braten: Is it possible to apply your method to the relations between perspectives or viewpoints? Can we, for instance, define the present as the co-presence of perspectives? The present must be something which allows for other perspectives to be within my domain, forcing us to think about the thickness of the present.

Atkin If we are talking about time, this is possible. Now events are fixed simplexes, and you can have simplexes intersecting each other, whereas points or lines might miss.

M'Pherson. I'm trying to relate this to Warfield's work, as an approach to structuring models of fuzzy situations. He uses graph theory, and control engineers find it appropriate as articulating the kind of thing they have been doing implicitly. He is also pioneering Interpretive Structural Modelling, an interactive procedure. The modeller may be the Town Council struggling with the best allocation of libraries or something. The computor asks the appropriate questions, and they eventually build a complete structure. Of course there are problems. You've got to train the user, the Mayor or the Town Clerk, and a complex problem takes a lot of iterations, a lot of time.

Atkin At the moment I'm doing a study on industrial relations for a company that shall remain nameless. They are making many kinds of product, as different as chairs and mayonnaise. There are 25 different kinds of job in different hierarchical levels, and several factories. The things coming out are traffic moving on the structure, which is defined by the relations between jobs and locations at various levels. Each product has a specific dimensionality, which is one less than the number of jobs that go into making it. If somebody comes along and alters the structure, say sacks all the carpenters, you get an immediate halt to the dimensional traffic which involves them. So you have to associate the products with peculiar topological properties which are essentially dimensional, and locate them in the structure. You'd not easily get this through a graph theoretical approach, which only joins things at the edges, so to speak.

Johnson Don't these languages bring with them some built-in assumptions about the way the world is structured?

Atkin Q-Analysis, apart from the interpretation, only uses a matrix to tell you that things are connected when they share things. Its not injecting anything into the world thats not there -- unless your observations are wrong. You will appreciate you can be wrong about sharing things if you are not clear in the first place. Which brings us back to hard data. You've all got to agree about whats there, and be willing to get it wrong a few times. Q-Analysis doesn't solve the problem for you, but gives you some concepts to work with.

Hogarth How long does it take the managers, the union representatives, and others you have had to deal with, to come to terms with the language, and to converse in it?

Atkin At a certain level I was surprised. It took about six months. They are much quicker than academics. Academics are particularly obstinate. (laughter)

Thematic Summary

Foundational: the problems of operating with soft or "fuzzy" data in social science. The need for hard data, defined as having set membership, and being susceptible to mathematical analysis.

Hard data achieved by common agreement and common instrumentation.

The practice of decision making.

The notion of probability in social science as often artificial - encouraging low-dimensional observations when high-demensional observations are necessary

Section 4

General Discussion I

General Discussion

Lewis: On occasions, in industry, commerce and elsewhere, I have encountered dissatisfaction with decision making. I have always managed to cope by providing people with "ordinary language algorithms" -- procedures for doing the job correctly. Often I have looked at "error factors" in the situation -- what it is that people are doing sub-optimally or incorrectly. But all these cases presuppose that the situations are likely to recur.

If you have an organization which engages in complicated decision-making of a kind which hasn't occurred before, then these procedures don't work. I want to raise the specific question of whether the kind of work reported here is orientated towards that type of situation.

I sometimes wonder if the models are coming along prematurely. There is so much data that hasn't been collected and analysed. Perhaps this should be done before we start to build models.

Pask It seems to me that if you winkle out this empirical data, you can make a lovely story which will be either descriptive or normative. The difficulty is to make it predictive. The best you can do with an accumulation of facts -- the data of a natural historian -- is to prescribe certain norms. Now, although in this context the exact nature of prediction is not fully prescribed, we can agree that a predictive theory is firm. It makes speculations which can be tested, which are hard. My model makes very definite statements about how people and groups work, and what they can do. These statements go out of the realm of natural history and norms, and into prediction. This is of particular value if the circumstances are uncertain, but is also useful if the circumstances are well-defined.

M'Pherson Let me set the ball rolling from the normative corner -- from the point of view of people who say "this is how decisions ought to be made, no matter what people are like in practice". We accept that

classical decision theory, Bayesian Theory, etc., is nice and tidy, but is very much orientated to single-objective criteria. This is not much use in practice, because the important things about complex decisions are not in the analysis. Decision trees, consequence analysis, scenarios, and so on are not at issue. The problem is to design proper value criteria for multi-attribute problems. Designs that bring actual human feelings and attitudes into the calculus, and that can assist people who are crying for help now. A fortnight ago the Minister passed me a list of "priority pollutants" with a request to rank them in order. Immediately there is the problem of objectives. Whose objectives? Concealed objectives. Implied priorities. Matching expected consequences with operational goals, when much of this can't be done in terms of money. Cost-benefit analysis goes out of the window, and we have to talk about subjective assessments, and the problems of measuring them. Then there is the problem of combining different measures, and assigning appropriate weightings.

These are problems which normative decision analysis has to get its teeth into, in order to become usable and acceptable. It must not do this in a vastly complicated way that requires decision makers and their teams to go on long courses in analysis. It must map the languages and signals they can understand immediately.

Hogarth I am concerned about these interactions, whether they are with analysts or with machines. I don't believe people have preference orderings before they tackle the problem. There is a strong interaction between the posing of the question and the objectives that are elicited. The models are said to be normative, relative to your preferences -- but this assumes your preferences exist. On the other hand Ron Atkin's paper was descriptive. But if you do Q-analysis for a company or a university, you impose structure on people by describing things. This poses important moral issues about the way in which this should be done, and the power of the analyst.

Atkin People usually describe in English what they imagine to be the problem. They say "there is a big hole in the wall of my front room where a picture used to be. I want the picture back." They need to understand that there is a lot of electronics behind the problem. The hole used to be a TV set before someone smashed the tube in. Having identified the problem in a different language, you can solve it. The language, like a good notation, may allow you to express the problem in a different way -- one you might not have thought of before.

Nowakowska. I am convinced that it is possible to construct such models. They can take into account variables that have been ignored up till now. My model is an attempt to incorporate psychological, social, and perceived social variables into the account of decision making, and to create a new kind of inference. The model is still limited, in the sense that you cannot take too many new variables, but at least you can insert some of them and see what happens

Nicolis: In many cases these cries for help are simply refusals to introspect. People should understand that we cannot make decisions for them. If you wish to study science, for instance, you do not go to the British Museum and read every book — then say "Now I am a scientist". You filter and select out. You use your nose — or heuristics. That is just maturity. The only thing we can do is help people discuss possible models of their internal deliberations. We can empathise with them as psychiatrists do with neurotic people. The analogy is sound. By definition, mature people who ask for help in decision making are neurotic in the grand sense. Consider a judge. No judge ever asks for help, and that is not accidental.

Atkin No. It is not accidental. Judges have a well-defined backcloth.

Braten Its defined by their robes.

Pask. They impose their structure on the environment. They are field independent rather than field dependent.

Corkindale One of the difficulties is defining objectives. Depending on whose perspective you take, objectives can change dramatically. I have always been struck by the definition of an air-traffic control system. It is a classic example of neatly phrased objectives that are useless for any practical purposes. Line one of the text book says that it is a system that produces a safe, orderly, and expeditious flow of traffic. In any practical situation, you can't be safe, orderly, and expeditious, all to the highest level, at the same time. Similarly, different people -- a pilot, a traveller, an insurance broker -- all have the same objectives, but rank them very differently.

Hogarth: Again, we have the problem of ordering alternatives. The problem that I see is that over time the analyst freezes the alternatives. The most important thing in this type of decision making is the generation of alternatives.

Pask The trouble is that some people have to deal with very complex situations by proxy. The reports are prepared, then taken and edited to briefs. These are in turn condensed, and then presented on one sheet of paper. This freezes the alternatives. It was intended to. The trouble is that it does not improve the decision making process.

The interesting thing is that if you collapse the whole spectrum of possibilities in time, the captain of a trawler will get into the same position as an air traffic controller. The fact is that nowadays we are up against situations in which this compression takes place, and we are not familiar with it. That is why I object to the one sheet of paper.

M'Pherson I think we can allow for compression if we assume on-line interactive facilities with a computer. If we have been able to define the class of problem — say decisions at sea -- then the rule book, the value surfaces, and any temporary parameters can all be stored. All that is then needed is to quickly punch in the parameters of the developing situation, and, presto, a set of alternatives.

Pask If that is the case, it is no longer a matter of decision but of action. It ought to be handed over to the machine anyhow.

Robinson I think we should distinguish the level of decision theory that we wish to talk about.

Let me illustrate this by anticipating Prof. Braten's talk. I found in his paper a most interesting juxtaposition between a scientific study of a referendum and an account of self-reflective groups. These illustrate the dynamics of the dichotomy that I am trying to get at. I see no reason why it should not be possible to predict the results of referendums, given correct principles and accurate simulations. If we are successful in this, the prediction may be negated by becoming public, as Popper suggests. On the other hand some entity may step in to restore unpredictability. The French Government attempts this by banning opinion polls in pre-election periods. A third, and more interesting possibility, is to build the predictability of given events back into society to form the foundation for a higher level of operation. With this last step, a theoretically object-group becomes self-reflective, and a higher level decision theory is called for. This initial problem for decision theory has been solved, but the general problems have not, and cannot be solved because they are always one level beyond where we are.

This is the importance of Gordon Pask's concept of Machine C., of THOUGHTSTICKER, of Kelly Grids, and of Ron Atkin's Q-Analysis.

They enable us to formalize and externalize, in a word, reify, areas of interest -- which then become uninteresting in terms of decision theory. Learning to fly is a real experience involving real decisions. Designing an autopilot involves the reification of those decisions. Flying an aircraft with an autopilot -- where predictable decisions have been built back into the system -- involves new, higher level decisions that are again of interest to decision theory. Until they are solved. This is rather like the paradox of the Power Set. Specific problems, such as the rank ordering of pollutants, can be solved. But there can never be a high enough level theory to solve the general problem set of which it itself forms a part.

Pask This emphasises the lack of difference between decision-making, learning, creativity, and so forth. They are all part and parcel of the same process. We mustn't get stuck with one kind of algorithm for extracting one kind of process, but allow for change that can be called decision.

Corkindale A lot of people are hoping for such rules, and hoping that decision reasearch will provide them, because of the information explosion. These days there is no limit to the number of on-line sensors you can have which will feed you all the information you need to know, and a lot of things you can't imagine why anyone should want to know. The feeling is that if we had rules telling us what information we need, and what we need to do with it, this would improve the quality of life by means of better decision making. Of course, that may be a 20th Century act of faith.

Pask It is a data explosion. The information created is a different commodity. The information is the resolution of the data, after the event, by a decision. There is a further sense in which information is the latent content of a decision -- and hence a case can be made for openness, which can be expressed in various ways. Given that the process is never finished, we can talk about a criterion of educativity, in the literal sense of the word (* "to strengthen the powers of the mind and the body" ED.). On the other hand, we can appeal to pragmatic criteria -- what is the use of this thing in real life. Oddly enough, a system which is in the genuine sense educative will have pragmatic value. It will loop back to the real world. In this sense a tautology has no value because it is self-traffic and cannot loop back to the world. Valuable decision theories must be open, not based on tautology or formal proof.

Thematic Summary

Foundational: decision theories as descriptive, normative, or predictive.

Decision as a choice among alternatives. Problems of defining objectives and imposing structures.

Well defined recurrent problems vs. ill-defined complex problems

The nature of problem complexity. Time compression/ data explosion.

Section 5

Decision : Foundation and Practice

Brian Gaines

Overview

Prof Gaines' paper in Volume II contains a deep philosophical discussion of the status of any "Decision Theory". It is argued, both with respect to logic and to psychology, that Decision is a process involving the valuation of possible or realised actions and that there are fundamental difficulties in expressing this process within the framework customarily known as a "Scientific Theory". For example, self and other reference is entailed, the framework is changed by some decision processes, etc. However, calling the formalisation of decision a "theory" or not, as preferred, it is still possible to rationally manipulate decision, and to recommend procedures.

Later on, Gaines examines the relevance of these arguments to man/machine systems, especially those employed as decision aids On the one hand, the availability of computers, especially distributed systems, radically changes the form and greatly enhances the potential benefits obtainable by aiding the decision process. On the other hand, the design criteria for man/machine systems must take account of all the philosophical or epistemological points raised in the paper, as a result of which the system configurations are likely to be quite unconventional, if the potentials for man/machine mutualism (or man/machine symbiosis) are to be realised. True, it is easy enough to set up locally useful predictive models, to display value contours, or perform otherwise burdensome calculations; none of which lead the designer to deviate from customary paths. However, these devices are relatively low powered (in comparison to possible though less conventional configurations). For the latter, a decision system must accommodate a decision process and subscribe to a paradigm based upon the earlier foundational reappraisal.

Commentary

Gaines: In the paper I attempted to run right through decision theory from foundations to practice, and the thread, obviously, becomes somewhat tenuous at times. This morning I shall go to the pre-amble, where I make the key points of the paper, and comment on them.

The first point is that the problems of a Theory of Decision are foundational. One of the important things coming out of this Conference is the frank admission of the weakness of foundations. The remarks made yesterday about "cries for help" and "neuroticism" make one wonder if this is not due to published works on decision, which appear to make quite definitive statements about what people should be doing. They say that they are useful, so civil servants and others feel that they should be able to use them. In the majority of cases they are not useful, and this is bound to induce some degree of neuroticism. The discussion that is usually left out is at this foundational level. Assumptions are not stated or are unclear. I have noticed this in Control Theory, for example. First it is said "let us consider the general case of optimization". Then it is said "let us assume that the system is linear", and then, "we will assume the performance criterion is least mean squares", and so on, until we get a nice mathematical result out of it. The implication of the first statement is that there is a general purpose technique which everyone can use. Then immediately there is a set of mathematical presuppositions which do not have anything to do with any particular class of problems.

The danger is that when one goes to foundations, one finds that there aren't any, for fairly fundamental reasons that I have discussed. A consequence of this is a very high degree of conventionalism in the foundations of decision making. The problem with accepting that -- and to some degree we are all forced to accept it -- is that we are left with a tenuous link to any correspondence basis for decision making. Much theory does not correspond in any general way to the real world, even when decision making is in the areas of technology that we have created --the real world at one remove, as it were.

At the most basic level, the presuppositions that people make about solutions to the problem are critical in the kind of solutions they come up with. Particularly strong is the assumption that there is one solution. If this is fallacious, the results are undermined right

from the start. If we assume that utilities can be well-ordered, we look for this. And if we look for it, we then find it. Yet the reality of well-ordered utilities, in any real situation, is dubious. In Control Theory, one of the most devastating results of the concentration of effort, ten years ago, on optimization techniques was that we ended up with elaborate control schemes -- which were optimal, but also highly non-robust. The slightest change in the plant ruined the control scheme. In terms of decision theory, the point that Gordon Pask made yesterday is crucial. One looks at the cover set as well as the set of alternative actions. One should choose a strategy that has a very high cover, even though it may not be optimal. This means that if one's assumptions are wrong, or the conditions change, one still has a chance that the strategy will work. The concept of "robustness" brings Sensitivity Analysis into decision making, although, like "stability", "robustness" is very difficult to pin down theoretically. On the other hand, if one looks at successful real life decision makers, they are generally the ones who have left themselves plenty of avenues of escape. A very good example at the moment is John West with their salmon. The same product is marketed under 10 different labels. This means that if one brand gets clobbered by a food poisoning scare, the other brands survive. Without this robust strategy they could go from market leader to bankruptcy in a matter of weeks.....

Interruption Robust, but unethical!

Gaines....I think ethics are out of place in decision theory. *

Going from foundations to practice, at this stage of the game we are obviously asking "what is the role of computors?", "how do they change what we can do?". Their role will not be so much in the working out of optimization algoriths and similar complex mathematical operations. Their significance will be as possible world generators; the simulation role of being able to follow through logical consequences, keeping track of all the possible worlds that can be generated, and evaluating them.

People are very bad at this. Even in fairly simple situations the "lookahead" problem can become overwhelming. The old logical axiom "if you know something, then you know its consequences" is just not

* This point is taken up in the discussion.

true in psychological terms. For instance, in the commercial situation, when people are looking at the current state of a market under equilibrium to foresee its future states, this is impossible, except through "intuitive feel" -- until one has a computer.

Of course, the answers that one gets depend on what one feeds in. People are forced to state their presuppositions, and to find out if they are reasonable. This is a valuable exercise in itself.

The other side of the computor story is that computors have been oversold right from the beginning -- and are still being oversold. This applies to all aspects of both hardware and software. Reliability, programability, storage capacity are all quite appalling. This is typical of a developing technology, and is changing all the time. The danger is in thinking the product we have at the moment is the product we actually want. Sneider, for instance, in his book on modal logics, starts by saying that elegant proofs in modal logic are no longer of interest because the computor can produce them for us whenever necessary. But if you try and find a computing system that is programmed for modal logic, you will find it doesn't exist -- to my knowledge. Even if you want a program for predicate calculus, you will find they are relatively scarce, and unable to deal with many real problems.

One of the most needed developments, fitting in with the generation of possible worlds, is the net generation database. Current databases, for all their sophistication, are really only storage mechanisms of the old pigeon-hole type. The most influential computer models assume that the database is in extensional form. It is not part of current database concepts to include inference generators and intensional objects - except in a few areas of artificial intelligence. Yet this is precisely what one would want in any real world simulation. There is a divergence between artificial intelligence work with intensional databases and simulation, when there should be a convergence. I hope this convergence will come about over the next few years. It is here that the other technical developments have their most interesting problems. The concept of an inferential database, one which can generate possible worlds and allow observers to interact with them, is a most important development. It tells one what technology is needed before computors can play any major part in decision making. It places an enormous stress on large high-speed storage systems rather than large high speed processors, and this is an entirely new direction for computing.

These are the key areas for discussion that I would like to bring up this morning.

Discussion.

Pask As I understand this, what you are really talking about is not possible worlds, but possible observers. This is the general epistemelogical sense of a possible observer who makes the world as some sort of cognitive image. If that is the case, you were not just talking about branching particle lines, in the sense of the possible world interpretation of quantum mechanics, but you were talking about branching observer lines. Here problems arise because you have several observers in the system. In computational terms, descriptions have to be properly expressed -- which is a thing computors really can't do. No matter what databases or Codd-like relational operators you use, computors only simulate. They do not have images, only simulations of images. This becomes very apparent when you try to interface the device with a human being. The reason is that they have only one locus of control, or, at most, a few parallel loci of control. Nevertheless, you could get away with this by just making a representation that really exists and somehow having many loci of control.

Another way of doing this has been proposed by Goguen; Gergely and Nemeti; and others. A familiar general example is a Montague Intensional Logic. The semantics of this intensional scheme accommodates a large number of different universes. One might call it a multi-universe world. These universes are called A1, A2, etc sharing a few syntactic features and the interpretation can be extended so that we have action values, rather than the usual truth values. Truth functions are lines touching at least one point in each universe, perhaps many.

One explicit extension is a stack of microprocessors assigned to A1, a stack to A2, and so on. This is not only a fascinating new branch of technology, but a rewrite of our usual Turing-based computational ideas; a model which looks like a tape of stacks. You then specify a finite state machine (FSM) which can address the stacks and operate on their contents (not just upon the cells in the tape like the FSM of a Turing Machine, but the stacks of microprocessors). But, next extension, this FSM could be any of the microprocessors in any of the stacks and the machine resolve conflict by passing control.

Finally, I'd like to bring up an important point in your paper -- the representation of analogy. Presumably you would mean that analogies written down in a language (which would have a semantic corresponding to a Montague Intensional Logic, for example, a subset of natural language) would be simply processors able to communicate with each other, but otherwise independent. Is this a correct mechanistic interpretation of something you put down in terms of category theory?

Gaines Your description would fit into the framework of analogy because the analogy category between the two would act as communication. At the lowest level, as the only correspondence, it would be the only communication possible. At the top level, the truth category, it would correspond to whether the communication was, in some sense, correct. There obviously can be structures like that which you can't interpret in machine terms.

Pask Not in present day machine terms. The restrictions are formal rather than in the hardware, since there are natural systems, "natural machines", that do this.

Gaines At the professional level, I wouldn't want to make these distinctions about possible worlds, because the observer bifurcation is very significant. It makes the difference between hard and soft science very complex. We can't have an ontological system unless we have some way of observing the variables within it. The assumption of a lot of philosophical literature is that variables should be observable for all observers. If we have observer bifurcation we have choice, but no such simple criterion. The models are liberalised in the extreme if only one observer must exist who can observe the variables. The only situations which can be excluded are those which no observer can observe.

At the computational level you are right. The big weakness is in simulating independent processes, although people have been talking about distributed processing even since Von Neumann. In the last 3 years the big firms have been working on this problem of database systems with content addressable distributed processing. The difficulty is in making such systems do anything whatsoever apart from invert matrices. We can talk in terms of the requirements for such a system, but we can't specify them.

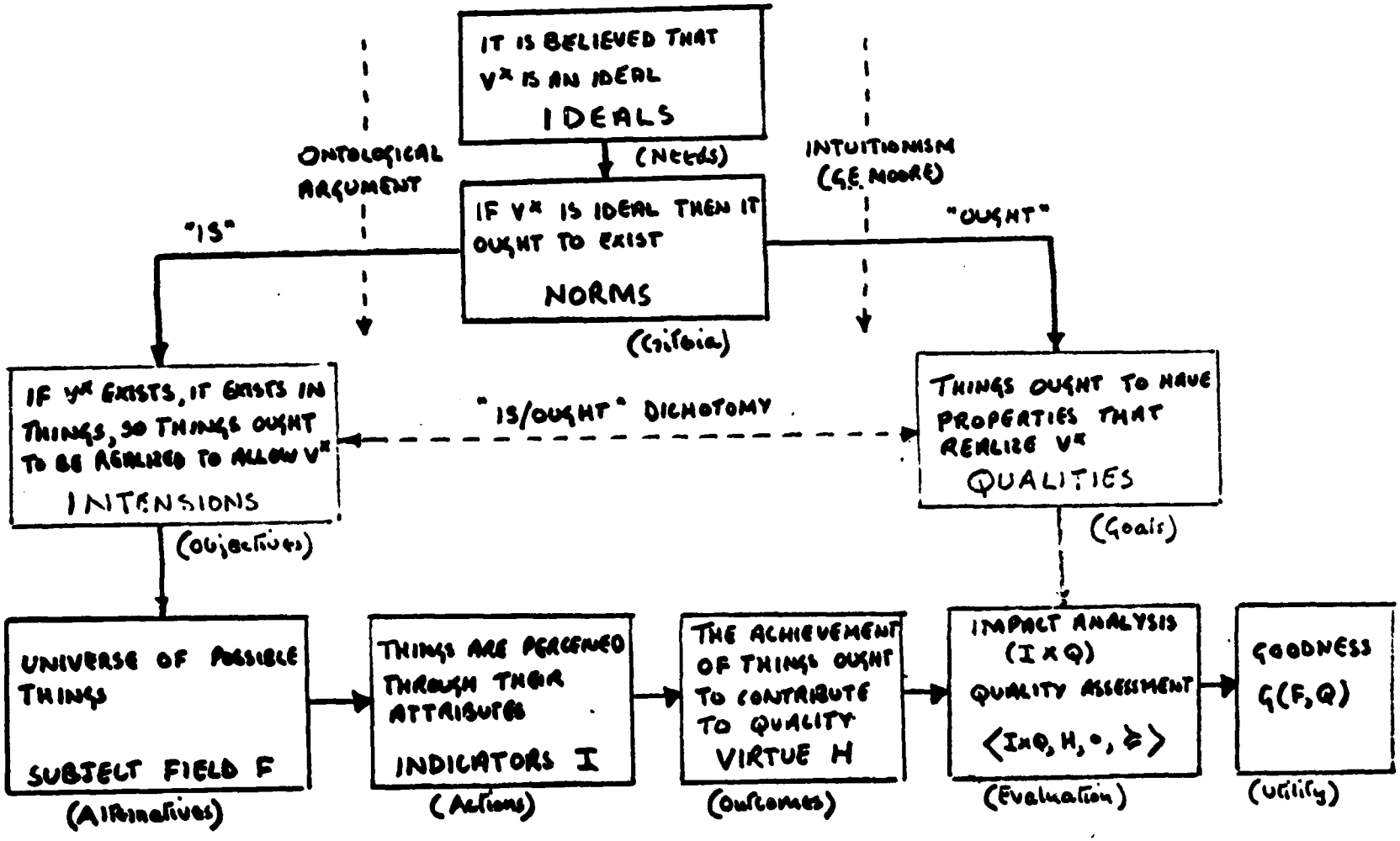


Fig 2: Philosophical basis to the value hierarchy . X quality assessment.

Pask This may be because we are loaded with the wrong formalism. The Turing-type program is excellent in its place, but cannot be carried over willy-nilly into this kind of technology.

M'Pherson. Brian Gaines earlier said that John West's strategy was robust. I grumbled it was unethical, and he replied that ethics didn't have much to do with decision making. I would like to challenge this. Ethics is the branch of philosophy concerned with desirable ends, and so are decision makers. Decision makers cannot avoid this, and, in fact, decision making is a part of ethics. It has been said that "decision making is the laboratory of ethics", and I think that is absolutely right. The meeting may take this as my position statement.

My position rests on G.E.Moore's Principia Ethica, and can be illustrated by this slide (See Fig. 2). Along the bottom row I've got the usual kind of process of decision making. The first box is the universe of possible things, or alternatives. They are constrained by the universe of feasible and permissible actions. Remember that many actions are feasible but impermissible. How can you avoid ethics if you deny that. If you deny that, you are not civilised by any criterion of civilisation that I understand -- although we may disagree as to what is permissable. Anyway, these things are perceived through their attributes. If, in fact they are to be useful or purposeful or desirable they ought to contribute to the quality that we think they ought to have. "Virtue" is a word that doesn't mean anything to modern morons, so I call it achievement now. Then we are into evaluation, and as a result of this we decide whether the thing is useful, cost-effective, or assess it by other quantifiers. On top of this is the value hierarchy, where we say that there are desirable ends. Each society defines what it believes to be ideals. If these ideals are defined, the implication is that they ought to exist. This is where we come to the intuitionism of G.E.Moore. If this ideal exists, then it exists in things. The properties of things can be divided into two categories. There are descriptive properties, statements of fact, which are completely value free. Then Moore says there are another set of properties, I call them qualities, which things ought to have. To say that a thing is good means it has a fair measure of the properties it ought to have.

Hogarth Are you saying "something is good" or "I believe something is good"?

M'Pherson It is both. This is the is/ought dichotomy.

Nicolis "Oughtness" is an evolving concept. It is different for a 3-year old and a 70-year old. It is hybrid between social standards providing environmental cues and unfolding genetic material. You cognise something as good or bad after a very long series of such correlations, and this gives ethics a relativistic basis.

Robinson Moore essentially finished off the job that Hume started. They took the monopoly of value judgements from the church, and gave it to another section of society. The is/ought dichotomy was instrumental in that process. This does not mean that the dichotomy has any truth in it. I believe it is completely false. We build judgements into the discriminations that we make in the first place. We don't make distinctions unless they are important to us. We have several sets of languages, all packed with fine discriminations, for talking about wealth — because this is important to us. The Eskimos have 30-odd shades of white in their language, because their world is made of ice and snow, and it is important to them. "is"s and "ought"s are both built into the basic act of constructing the world, and are not seperable. You only get stuck with the dichotomy if you consider a world of objects and forget the more important world of actors.

M'Pherson I found the distinction useful because I am asked "what is the value of knowledge?", when we have to decide to spend taxpayer's money on it. It gives me a structure. This is why I make such a fuss about decision makers sorting out their objective trees, with ideals at the top, working steadily down to operational, realizable goals

Johnson Would it be oversimplifying your argument to say that decision involves values?

M'Pherson That's OK. If we want efficient ways of coming to a decision we have to find out why people make the decisions they do. I say we should look at the culture, the church, the parents, the school -- every decision is going to be a reflection of the value system.

Gaines I originally meant that an absolute ethics has no place in decision making. If I can inject whatever values I like into your top box, I would accept that, because one is dealing with the values of the system one is organising. However, an absolute ethic, forced on one from outside, which simply announces something is unethical, really has no role. It is an invalid starting point for studying decision making. This is partly because a lot of decision makers don't act on this basis, partly because there are individual decisions, made in terms of personal values, that go completely against the ethical values of society.

In rational decision making, externally or socially imposed ethics have the role of constraints. I can put costs on them. If I contravene a particular permission, it is not an absolute where I immediately drop dead through God's wrath. In fact, I pay a penalty of a certain kind, and these penalties can be bought into my decision making framework. There is also an epistemic logic to this, because the penalty I pay depends on whether anyone knows I contravened that permission. If one says that ethics is composed of value systems outside my value system, then they come down to constraints with costs of contravention -- a part of my environment. One's feelings about morals must not be confused with the realities of situations that actually exist.

Thematic Summary

Foundational: weaknesses are necessary, not contingent. "Conventionalism" and the role of presuppositions. "Robustness" as a strategy.

The role of computers: possible world or consequence generators. Weaknesses of current technology, importance of inferential databases.

Psychological concommitants; the inability to cognize multiple consequence sets.

Possible worlds and possible observers; the importance of distributed processing and inappropriateness of Turing-paradigms in simulation

Analogy in communication

The role of ethics in decision making

Section 6

Competing Modes of Cognition and Communication in Simulated and Self Reflective Systems

Stein Bråten

Overview

Prof Braten's paper in Volume II is a theoretical contribution backed up by ample empirical data and on a fitting methodology. The theory as well as the methods are conversational, and similar to Pask's "conversations", although the notation employed is a little different.

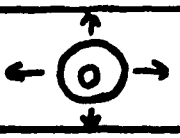
Braten refers to his theory as Dialogic since it is related to a dialectic philosophy and the hermaneutic epistemologies.

His arguments are complex and it is impracticable to do justice to them in a summary. However, a few points can, perhaps, be highlighted. One of these is the notion of "meaning tightness" (comparable to Pask's "understanding"); meaning tight episodes are interleaved in conversations with people or coherent groups of people. These episodes fluctuate in the course of a typical conversation with periods in which the participants are not in accord, and do not share meaning even though they may use (at face value meaningful) ethical terms. Another point is conflict resolution, or potential conflict resolution by means of a dialogical progression, in the spirit of Herbst and Habermass amongst others.

Commentary

Braten. I shall link my comments to the Starbase example that Gordon Pask gave us . In this system participants assume the role of mercenaries operating spacecraft to maintain trade between four starbases. They are informed about strange entities, "Klingons generated from "inner space", which consume freighters and their contents. We may also see Klingons as a part of the overall ecosystem -- when too much energy is dissipated in annihilating them there may be a dramatic fissure in space, which makes it necessary to try to knit together the environment again.

Let us take the game a little further. Imagine the participants, A & B, have acquired different world-images, either though background socialization (church, school, and so on) or through the experimental briefing. Let us assume that A is accustomed to think of space in terms of expanding doughnuts. He is accustomed to think of strange entities as aggressors, and to think of co-actors, thus creating the possibility of I-Thou units. The other mercenary, B, is accustomed to think of space in terms of ever decreasing sugar-lumps. B conceives of strange entities as potential collaborators, and thinks of co-actors as objects for manipulation. He tries to create I-it relations. So A and B are equipped with different pairs of spectacles. Let us call these the p-spectacles of A and the q-spectacles of B.

	A (p) 	B (q) 
space		
Klingon	conflict	co-operation
The A/B Dyad	I-Thou	I-it

Let us assume that the system offers A and B "raw" data which compels the two mercenaries to apply their spectacles, not only to search for information, but to process and interpret it. We can now predict, especially if A and B are scientists, what they will do. A will use his p-spectacles to filter out some of the raw data if it cannot be interpreted and assigned meaning in terms of his only available means of processing. The same goes for B. They do this because they have to create an order in this universe. They employ a reduction principle, and the basis for the reduction is their spectacles.

We also know that they will employ another principle — that of Cognitive Consistency. They will resolve psychological contradictions through indirect processing in the ordering of their universe. When they obtain feedback on actions taken as a result of these images, they may be faced with inconsistency again, and will resort to different kinds of inconsistency resolution.

Then a fissure occurs in space — a critical instance -- and they are forced into interaction. Again inconsistency will arise. Each will soon realize that the other is equipped with a different set of spectacles. There are two modes of inconsistency resolution. The most natural, especially if they are scientists, is that A and B will both accept the premise "if you are right, I am wrong". This fits with Brian Gaines' paper, and is the basis of many famous scientific disputes. If they are more sophisticated, they may attempt another way out of this dilemma. They may work towards a synthesis — and they may succeed. But what is a synthesis? It is a strange attribute of many theoretical syntheses that elements within them lose their identity. They lose distinctions. What remains is a more general, but emptier structure. Everyone can agree on it, but it cannot be turned to practical use. In this case, we can imagine a synthesis where the Universe expands at the edges and contracts in the middle, where Klingons are neutral, and where manipulation and co-action are both called interaction. But the universe <u>has</u> been constructed with different regularities -- both the p- and q-spectacles apply. What will A and B do?

I have given this illustration because it is easier to demonstrate ontological assumptions with a constructed world. We can see that reality — or at least the psycho-social reality we know -- is capable of exhibiting conflicting regularities, laws, and so on.

So what happens if A and B are forced to interact? They must develop another kind of image, which I shall call R(p,q). This reflects the competition between the tendencies, or the images, depending on your epistemology. It must also contain a specification of the conditions for shift between these conflicting modes. There may be two basic types of competition between such conflicting modes. When one mode is active, the other may be suppressed. I call this shift operational incompatibility. There is also a more complicated relation. Both tendencies may operate in the system in parallel. This is a competition for dominance. There are two subcases here. Both tendencies may operate on the same variables -- in which case one talks about conditions for the validity of the images -- or they may operate independently. Depending on the level or the conditions, one may have, for example, an I-it or an I-Thou interaction.

How are we to describe these reflective R(p,q) images? They are not "particular statements" in Popper's sense, since that would mean they could not have the status of a theory proper. Nor are they statements of a theoretical synthesis, for reason that I have already given. The R(p,q) image is an image of the relation between images that preserves all the distinctions of the original images. This conception has been forced on me by the realities of my empirical work with psycho-social systems. It is necessary to approach these systems in a manner that allows us to create these R(p,q) images. This may be done through traditional computor simulation. Alternatively, it may be done less "scientifically", in the traditional sense, using the self-reflective paradigm. One forces the system itself to participate in these self-reflective circuits to develop images of the conflicting tendencies in the reality of which they are a part. But this creates a number of methodological problems. The competition between models always involves the competition between two sets of spectacles. The comparison will refer to a "database", or a source of data -- but the source of the data is already infected by the spectacles. Thus, a priori, the possibility of comparing competing models is partially destroyed.

But there are other, more difficult problems. The definition of "level" changes whenever the spectacles are changed a little. I said that these two images somehow concerned the same level -- the level of A/B interaction -- but this is not quite correct. I cannot for instance seriously apply the same kind of level when I apply the I-Thou perspective as when I apply the I-it perspective to the relation.

The most serious problem is the question of time. Whenever you change your spectacles a little, you have to re-adjust your definition of levels, and you have to re-adjust some of the "hard" descriptions you apply. As a consequence, even your definition of time has to be adjusted. But if you have to re-adjust your definition of time every time you change your spectacles a little, this creates yet more problems for the construction of R(p,q) images.

I will now briefly list the kind of studies I have been involved in since 1973. I started with one pair of spectacles — and had some success. But gradually, on returning to the data, the comparisons between different models, and between models and referent systems, I was forced into this way of thinking.

Study	Method		
	Empirical	Simul--ation	Self-Reflective
1. Moral dilemma processing dyads	x	x	(x)*
2. Map dilemma processing dyads	x	(?)**	x
3. Processes in a referendum controversy at the national level	(x)***	x	
4. Processes in a referendum controversy at the community level	x	x	
5. Self-discussion groups.			

Fig 4

These studies are all mentioned in my paper , with examples of competition between modes or tendencies, so I will not go into them now. I will only say that the self-reflective discussion groups are not "scientific" studies in the usual sense, although they do create traces on videotape which may allow for later study.

* A subset of these dyads have been subjected to video-playback, but the results are not yet fruitful.

** Here we are working on a computer model, but it is turning out to be so terribly complex that I must put a question mark here.

*** Here the empirical studies and surveys were done by others.

Discussion

Nowakowska Do you have an example of these self-reflective groups that you could show us?

Braten. I thought of that, but it would take two hours to demonstrate the processes. They cannot be given in a snapshot.

Nowakowska I know of a similar technique in psycho-therapy. It is called psycho-synthesis instead of psycho-analysis, and is a kind of retraining. After the session the person is faced with himself to interpret and make judgements on. He is also asked to change his name, and to say that he is not the person there with that unpleasant list of attributes. Instead he is now a person with new, pleasant attributes -- and this is even announced on local TV. (laughter). This retraining provides a new framework and set of descriptions for that person.

Braten This is interesting, but I think the difference is that in my groups you have the possibility of reflection on reflection on self-reflection. This is depicted in Figure 6 in my paper, and is the limit of what can be concurrently processed by the participants. In addition to the interesting things which occur in terms of interpretation, these self-reflective feedback circuits also reveal things about our notion of time. One decides to return to a specific incident. "Well! It was before that", "No,no, it was after". Or the incident may have "stretched".

Another revealing aspect of these studies is the relation between meaning-tightness and the amount of tension and anger in the system. This is precisely revealed through the playback. Tension and anger occur when one is attacked -- but it is revealed afterwards that one felt one was attacked on false premeses. There was no state of meaning-tightness. I was attacked for something I did not mean, criticised for something I did not assert. It is not, as we usually believe, that we become angry because we disagree in terms of opinions. We become angry because we do not understand each other. If this is resolved, if meaning-tightness is established, we may have a tough quarrel -- but everyone is happy.

But meaning-tightness is a shifting condition. Unless you then loosen up the connectedness, it is impossible to go on. There is a continuous shift between creating and destroying distinctions. The notion of meaning-tightness between at least two participants is important, but absent, in semantics and semiotics. You cannot discuss sign and reference without at least considering two minds and their connectivity -- your list of agreements are also the prerequisites for disagreements.

Pask I think this is very realistic. This business of looking back, resorting things in a fairly regular but intriguing way, occurs if you look in detail at almost any data on meaningful individual or social learning. The use of a recording medium encourages the creation of a new distinction. It would be especially nice if you could slot it back to an appropriate moment -- that's the difficulty you are talking about with time, of course.

Nowakowska It seems to me that you are giving the ability to return to the past, or strengthening the short term memory. They can return again and again until they are satisfied. This is not the case in normal dialogue, where people quickly forget their exact formulations.

Pask They may forget exact formulations, but they remember in a thematic way. Apart from these formal means, you can also establish understandings, or reproducible concepts, in other ways. You can do it by getting someone to enact a key role in a story, and this will not be forgotten -- although it is correct to say the details are not remembered. But they are not forgotten in the sense of being eliminated. I think they go into an ongoing part of unconscious activity. They become the bursts of anger. They are reliably stored as a continuing antagonism -- until an understanding is realized.

Braten This is consistent with the notion that to forget is never to erase; it is to decrease the probability of activation, or to increase the probability of activation in a disguised form.

Helme This phenomena is related to normal changes in the memory trace towards a better structure, or tightness. In a sense you are preventing the assimilation of this.

Corkindale This technique is very popular in therapy, where, by definition, the normal structuring processes must be broken down in some way. Here the therapist acts as a kind of store or mirror, with an occasional reminder of what you have said. TV could act as this sort of memory. More interestingly, you could have an interactive computor system where you talk to yourself — but one of the "selves" is very logical and accurate.

Pask We have run a similar study on decision and safety in driving. We used simulated traffic situations, like overtaking, and a modified version of Laing's IPM test.* We presented subjects with a sequence of "traffic situation" slides, and they were required to say what they would do, what the other driver would do, and what the other driver thought they would do. Later in the sequence we reversed the situation by reversing the perspectives and the labels on the cars -- without telling the subjects. They then saw the world from the point of view of what used to be the other person, and this gave us an IPM match between perspectives. This is the most reliable index I know of safety in driving.

Braten Theories about competition between organizations within the personality can be placed in this context. You could call an image of a healthy organization a p-image, and an image of a neurotic organization a q-image. The point is for the patient to develop a reflective (p,q)-image. The first self-reflective loop would, at best, be able to create some negation of an existing image. But reflection on the reaction to this dual image is necessary for an effective therapy.

Robinson At this second level of self-reflection, do you think it may be necessary to use a different form of reification of the processes people have been through? This relates to the possibility of determining limits to the complexity of unaided constructs, whether they are being used by individuals or groups. Brian Gaines mentioned that decision problems arise in areas we have created, notably the "data explosion" and international relations. Now self-reflection is something that people do all the time, but unaided intuition is incapable of dealing with these larger situations. To get to a higher level situation we have to externalize ourselves as artefacts, clone ourselves if you like, in order to co-operate with ourselves.

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*R.D. Laing,H.Phillipson,& A.R.Lee. Interpersonal Perception Tavistock.1966

With unaided intuition you can get to about the second level of Laing's IPM hierarchy -- what the other driver thinks I think he is going to do -- although thinking like this is hard work. With video-playback, you can get to here, and a bit further without too much difficulty -- you can reflect on your reflection on self-reflection. But even here there are obvious limits. Although logically, in principle, these hierarchies can be extended indefinitely, in practice they cease to be meaningful after the first couple of levels. To get to the n-level, where decision can match the interactive possibilities of the situation, we may have to climb a ladder of such artefacts. One obvious item would be a concurrent simulation of the predictive, mechanical level at which your system was operating. The video-system would give you a couple of further levels. You might index your position in the reflective hierarchy by sound signals, and so on. Different modalities may allow us to use different senses to interact with ourselves back at the different levels.

Braten You have to be careful that the increase in the number of modalities does not compete with incrementing the number of levels -- although this is certainly a possibility.

Pask You would be building in punctuation marks, which people are very bad at doing. The modalities would be distinctions, but reserved as punctuation marks. It is a pity that control display systems are so rarely used in this way.

Nowakowska Does self-reflection increase meaning-tightness in the systems you have observed?

Braten One has to be careful about this as it is related to the number of self-reflective circuits. The map-dilemma dyads, with only one circuit, were very much concerned, operationally, with meaning-tightness. In general, my conjecture is that self-reflective circuits increase the capacity of the system or group to make distinctions. They move more rapidly from one position to another, but in order to move they have to create tightness -- but always in the context of competition.

Nowakowska Did you use any linguistic analysis, or sub-model of how a person answers questions?

Braten I did use that kind of approach for the simulations. They went very nicely for one subset of dyads, but poorly with reference to another subset. That was one of the studies that pushed me through the developments I have described today, and -- to return to my starting point -- I would say that I am now continuously afraid of one model.

Thematic Summary

Foundational: ontological assumptions underlying the creation of "hard" data. The possibility that "psycho-social reality" (the world) embodies contradictory tendencies.

Methodological and epistemological problems of "joining" multiple conflicting perspectives, or creating R(p, q) - images. Difficulties in achieving common agreement by synthesis.

Meaning-tightness: a para-semantic relation between participants; and anger, and therapy.

Psychological limits of self-reflection: ways of extending these limits.

Section 7

On the Spontaneous Emergence of Decision Making Constraints in Communicating Hierarchical Systems

John S Nicolis

Overview

Prof Nicolis' paper in Volume II is primarily concerned with the interaction between two (or more) systems, each having two or more levels of hierarchical organisation. The systems are playing a game such as "Chicken" or "Prisoner's Dilemma" in an iterative manner; possibly switching from one game (payoff matrix form) to the other.

At each of the hierarchical levels, there is a stochastic process and parameters are changed in order to maximise a joint figure of merit.

The interaction and the interacting systems are computer simulated, using an ingenious doubly stochastic technique to introduce a desynchronising jitter (overcome by action-induced local-synchronisation).

Since the system is adaptive, there are inessential bifurcations in the system trajectories; these are resolved to maximise local stability. However, Nicolis shows that there are also and necessarily, essential bifurcations that have the effect of changing the original state description, or, equisignificantly, of inducing the originally assumed hierarchical organisation

Decision Making, at least of a significant kind, is modelled by the resolution of these essential bifurcations.

Commentary

Nicolis. The best way to present my thesis is reacting to the orienting questions. In the paper I tried to predict the degrees of synchronisation of the behavioural mode turnover of two hierarchical systems involved in a conversation. This assumes the parametricisation of every hierarchical level under the constraint of maximising a function which I call the "Joint Figure of Merit" (of the system), which is derived from neurophysiological, biochemical, and psychological raw material. At each hierarchical level the "Joint Figure of Merit" balances the intrinsically conflicting factors of homeostasis and transcendence in a preselected fashion. In other words, there is conflict between fluctuating environmental cues that bombard the organism (demand may change) and prestored internal dynamic patterns. Maximisation of the "Joint Figure of Merit" amounts to establishing the best code, map, or transformational rule relating the system to structures at the lower hierarchical levels and (higher) cognitive levels that are, in principle, unobservable.

The first question concerns "What is decision making, how should it be viewed, and are there multiple perspectives?" My reply is that decision making is a multi-dynamic, discreet, stochastic process which manifests itself via behavioural mode turnover - otherwise it would depend upon hidden variables. However, it would be oversimplifying to study such a process in "Skinner boxes", using successive steps of exploring, modelling, and controlling. I have adopted a holistic point of view. I try to present decision making as a spontaneous and inevitable by-product of continuous dynamic interactions between different hierarchical levels of an individual and a partner system; usually this partner system is the environment. Decision making is a succession of instabilities leading to bifurcation (a "Decision"), and involves a continuous dynamics. Bifurcation involves an instability of the hitherto stable steady states of this dynamic system and then a transformation of the system, at the proper hierarchical level, until it lands in a new steady state which is necessarily of a different construction, ie. there is a differently specified state space. During the journey in between these conditions the system is unidentifiable - at least, in terms of a description in phase space.

A few words about the dynamics, the branching qualities, of coupled non-linear differential equations are needed in order to explain why I envisage decision making as a series of bifurcations, as a set of instabilities involving singular points in a set of non-linear differential equations emulating my system at the level involved. Eventually, I have to undertake the hard job of identifying variables and parameters, and the way in which variables are coupled. In the vicinity of any steady state, these variables assume strictly determined, but flexibly bounded, phase relationships. If there is an instability these flexibly bounded relationships are dissolved, and the system "resynthesises itself", as it were, in the vicinity of a new steady state.

Concerning the nature of decision tasks, and how they should be structured; the whole point, in my opinion, is that the task of the partner-system is not specified at the outset, but is to be discovered inductively in the process of communication. The task is to learn, to use trial and error and see what happens. The individual reacts to what he cognises, and by his reactions he modifies the environment. So decision tasks, essentially, are the implementation of learning proceedures.

My reactions to the following questions is joint. Is the individual overlooked? How do we define his role? What variables should we include? What is a "good" decision, and how do we evaluate it?
Traditionally, good decisions are rational. Yet the concept of rationality is an expanding one. By that, I mean that appropriate evaluation procedures should include a reconciliation of individual and collective rationality. This complements the inductive paradigm of strategic game playing with an iterative paradigm (despite the paradoxes that can be generated!) Inductive reasoning does not aim at validation, it aims to put forward plausible isomorphisms, plausible arguments, or to provide insights about basic assumptions which deductive methods take as departure points.

A good, or rational decision maker always acts flexibly, but with hesitation and inhibition, and with guilt and remorse -- or "responsibility" as we may call it. I am convinced that in most cases the decision maker knows what

are the good decisions. What usually prevents him from implementing those rational decisions is either an overstrong conflict, leading to behavioural paralysis, or else no conflict at all, leading to uninhibited, impulsive behaviour.

Can we improve decision making? Here again I think that decision is contextual. It manifests itself in instabilities prompted by specific uncertainties and conflicting factors. Relevant parameters exceed critical values, thereby destabilising the hitherto stable steady state. Decision making cannot be understood, much less improved, without taking into account the decision maker and his psychopathology. In this context I advocate a type of growing-up. People should be induced to have the proper amount of responsibility, leading to inhibition prompted by guilt and remorse. In any complex system inhibition is a virtue.

The most pressing unresolved issue is our lack of heuristics; a point developed in the model described in my paper. Informally we have too many ways of mapping levels of emotions to levels of cognition, and levels of cognition to levels of behaviour. Which is the best map? In the absence of heuristics we have to perform an exhaustive search - and a lifetime is not enough for that. Nevertheless, people behave all the time, and one might say that their performance is suboptimal - but by how much is it suboptimal? This cannot be defined unless we know the optimum solution. We cannot perform an exhaustive search of the mappings between emotion and cognition or cognition and behaviour to find the best Figure of Merit; leading to the best synchronisation between individual behavioural mode turnover. The synchronisation is not intended to be perfect, otherwise the two partners would be endowed with some tropism, orientating to each other like automata. On the other hand, we must have some synchronisation between behavioural mode turnover, or there is no conversation at all. The best map, maximising the Joint Figure of Merit, is the one which compromises between these two limits. Eventually it leads to a modest synchronisation, but this is quite natural. (All communication involves delays, and so on). Heuristics are important because we cannot perform an exhaustive search.

Without heuristics the only rational attitude is Existential Despair. To illustrate this, in my model we can consider 40,000 possible maps. One method I tried was to select 30 by a Monte Carlo method, and then to perform an exhaustive search amongst these for the best Figure of Merit. This is not a very satisfactory method. A second approach is to consider the emergence of a new hierarchical level in one of the two partners. From this new level inhibitory feedforeward control commands can constrain the existing dynamics and limit the behavioural repertoire of the partner concerned. Again this is not satisfactory, although it is realistic in the sense that people always act suboptimally.

Another method, using computer simulation and some theoretical considerations, is to change the game. If you use a more sophisticated (and more paradoxical) game, then you get rid of the mapping problem. You can spot the best feedforeward controls in a finite number of steps, and this will ensure the best Figure of Merit and the optimum synchronization.

To summarize, the concept of interacting systems as the site of decision has been computor simulated, with the interaction considered as conversation-like. Each system, or "player" has a "game" (similar to "Prisoner's Dilemma") with an appropriate payoff matrix. The lower level in each system selects moves. These are represented by a stochastic process in a probabilistic state space. Adaptation gives rise to changes in the "fixed point" of an ensemble of states as one system adapts to compensate for moves in the game made by the other.

The higher level in each player system operates correlatively on the lower level ensemble, in addition to the interaction between ensembles in the lower levels of each player system. The adaptive transformations in either "player" lead to a behavioural trajectory; this is <u>not</u> decision but <u>adaptation</u> because the overall condition is compatible with the particular state space. Hoevever we also see that systems <u>must</u> always encounter situations that are not stable against such compensatable bifurcations.

In the case of essential bifurcations, the higher levels of the player systems must mutualistically change the co-ordinates of the state space, the frame of reference. This is decision. It is also a non-trivial variety of self-organization, while adaptive changes are necessary but trivial self-organization.

There is ample physiological evidence that the concommitants of decision in brains give rise to neurochemical changes and differences in the balance of transmitter substances. There is congruent evidence that emotion arousal, etc also involve changes of this kind.

It must be emphasised that if we have the techniques to construct this distributed view of decision making as hardware, then the model which is simulated is consonant with the neurophysiology and psychophysiology of the biological brains involved in decision making.

Discussion

Atkin You talk about the appearance of a new hierarchical level, and I find this somewhat arbitrary. It seems to be brought in later, as if the system were looking for an excuse to explain a bifurcation it couldn't otherwise understand. The sort of hierarchy that I talk about is there before you begin, but here it is produced by the system. I am not yet clear why these new levels have to be produced.

Nicolis Strictly they don't. It is certainly not inevitable that they will appear. I consider them as a dormant potentiality residing at the hitherto highest hierarchical level. If we consider the case where this level is perturbed by external fluctuations -- if the environmental fluctuations are sufficiently forceful, there is a definite probability that the organism will either collapse or go up a level to accomodate them.

Hogarth You discussed the necessity of heuristics and their relation to trial and error learning. Could you go over that again?

Nicolis In terms of my model I pointed out that there are a tremendous number of ways to map one level to the other level. We cannot try all possible maps in order to find the one which maximizes the Joint Figure of Merit. Suppose the world were uncharted, but I would like to travel the globe to find myself the best city to live in. I cannot spend ten lifetimes on that. I could stay at home, and try to optimize my life within my own city. But that is not very satisfactory. Perhaps I can do better. I might even be able to achieve the best solution if I could discard a number of sub-optimum solutions beforehand. On the basis of certain information, I might say "Under no conditions will I live in Bombay or Bangkok; nor do I wish to live in Queens or the Bronx." But the heuristics are lacking. We don't know how to provide them. I am suggesting that by building a new hierarchical level (which, by the way, is done for another reason), and by precipitating feedforward control commands on the hitherto highest hierarchical level, you constrain the dynamics. Your heuristic shrinks the repertoire, so to speak.

Braten Would you say that your Figures of Merit, which are somehow records of simulated dialogues, could all be described as slightly pathological? There is a tendency for one or both the participants to be hyperactive for most of the time.

When I construct computer simulation models I am always very suspicious of pathological behaviour. If I see too many indications of it then I return to the original model. If this behaviour can be considered pathological -- I am not asserting it -- then it may be that your model is not quite able to cope with the two conflicting underlying tendencies that you postulate. When you implement functional relations between these two tendencies, and shifts or jumps between states, you have resorted to two different strategies. You say that movement between states is a function of the tendency towards self-transcendence and of the tendency towards self-reproduction. You also say that when you end up in one of the almost absorbing states -- a state which could have been absorbing -- you allow for jumps. But you have to add another kind of mechanism there.

Now, if you had accepted that these two competing tendencies required two models of their own, you could have asked more specifically what were the conditions under which one of the models would be valid. I am of course leaning toward the kind of logic I introduced earlier.

Otherwise, one can say that your model does produce oscillations, and there is evidence of a shift between tendencies. I have some records of laboratory groups which produced similar oscillations, and this could accord with the type of process you have constructed. The difference is that the movement was between the tendency towards inter-personal consistency -- a very high degree of closeness existed -- and the tendency to create a new complexity, or variety, and thus increase the distance between the participants.

Nicolis Let me start by answering your first point about the rather suspicious tendency of the partners to get into the euphoric state and stay there. This is due partly to the degree of accepted parametricisation, and partly due to poor quantization at the level of parameters. One parameter in particular -- concerned with the cross correlations of the Markov Chains -- was only controllable to within plus or minus 10% without running into super computational complexity. I accept your point that the model is limited in this way.

M'Pherson I have one or two questions, because, at this moment, I am not able to suspend my disbelief in your model. You said that every decision maker knows a good decision, but is prevented by circumstances or something else from taking it. I think this must be a very special case, because I can think of all sorts of counter-examples where the good decision is not obvious.

Nicolis I mean obvious in the personal rather than the social sense. Acts of bad faith, as the existentialists put it, form a whole category of bad decisions where the good decision is known.

M'Pherson. Your system, in its assumptions about the lower operational levels with the Prisoner's Dilemma game, assumes perfect information. This raises the question of "what is a good simulation?".

Nicolis I do not think it is invalid to assume two moves, like C and C in Prisoner's Dilemma. These could be concentrations, either above or below a certain value. I have given examples for the sympathetic and parasympathetic nervous systems. The model is just an isomorphism. What can be said about two pairs of hierarchical levels in the model can easily be extended to millions of pairs of such levels.

Pask I don't think these objections have much force when we are talking about simulations rather than actualities. Nor was this wide explanatory power claimed for the model. I find it fascinating that, even in such a deliberately simplified structure it is still possible to see certain emergent type of phenomena -- which I believe can rightly be identified with decision. The simulation does not, and cannot, resemble the act of decision in planning (or in the organism at the neurophysiological level). It cannot because it has been deliberately simplified for tractability and repeated iteration. The important point is that essential and inessential bifurcations are realised inside a system with the minimum number of assumptions. The essential bifurcations, which necessarily change the state representation, can be identified with acts of decision. In contrast to this, adaptation and conditioning, possibly learning,

occur as a result of bifurcations which can be accommodated within given state descriptions. This is a distinction of real interest, and has the same appeal to me as a somewhat different distinction "ultrastability" and "stability" in Ashby's "Homeostat".

Nicolis I would like to repeat that, in my opinion, decision making is not a neat, cool, intellectual and intelligent choice between alternatives that are given a priori. Rather it is a dynamic phenomenon properly belonging to that statistical mechanics of non-linear irreversible processes. We are dealing with multi-dimensional stochastic problems. Catastrophic phenomena like bifurcations create uncertainty in critical value parameters, and new and unsuspected structures appear in space and time. Unsuspected is a good word here because both the decision maker and his environment are taken by surprise. I am convinced that this is the proper mathematical domain in which to tease out paradigms for decision making. But the relevance is in isomorphisms. My paper is an exploratory venture and a step in this direction.

Thematic Summary

Foundational: definitions of decision making; the unpredictability of essential bifurcations.

"Good" decisions: the possibility of improving decision making.

The necessity of heuristics in decision making.

The nature of decision models: limits and advantages of simulation.

Section 8

General Discussion II

Discussion

Hogarth As a psychologist, I was not familiar with many of the references that have been made here. I have tried to pick out some of the key ideas which I found interesting, and which I hope to incorporate in my work.

One of these was the notion of psychological balance and cognitive consistency theory, both nicely represented in John Nicolis' and Stein Braten's work. It was important in their specific systems, it is important for those like myself who are interested in the processes of judgement, and it is obviously going to be important for decision theory in general.

Another interesting aspect has been the talk of consciousness-raising. Gordon Pask's paper described situations in which you were confronted with problems that required decision -- and in a sense this was train ing in consciousness-raising. Ron Atkin described the presentation of structure to people. This explicitly dealt with consciousness raising, since allowing people to see the structure they are in is a consciouness-raising exercise.

An item which bothers me, because it does not fit in with my preconceptions, is the way in which the notion of time has been dealt with. With reference to processes of judgement, time as a behavioural dimension has not had adequate attention. To a large extent we live in a series of time series, each with a high degree of auto-correlation. At the descriptive level this is important since much cognitive activity is adjustments to previous states -- until you get a bifurcation. I would like to see our work take more cognisance of this phenomena of continuous time series.

Atkin I don't think time series are very good at predicting things, because you are appealing to clock time which is zero-dimensional. I would like to see a graded time series. For instance, the algebraic direct sum of a number of time series would be graded as you went from one component of the sum to another. This is an essential part of Einstein's General Theory of Relativity. It's the number of dimensions that is the point, and if you had said that I would have been delighted.

Clock time only allows you nodes, zero-dimensional events, graphs that go from point to point. If your events form a big, multi-dimensional simplicial complex, it only allows you to go round the edges, whereas you ought to be going through the middle.

Hogarth With our limitations, most of the time all we can do is go round the edges.

Atkin But it often works wrong. Time drags or it flies. We get old and still behave like children. How long will it be in minutes and seconds before I grow up, or before my business is a success? These questions can't be answered in clock time. You can only pretend to answer them in the way that you answer questions in Newtonian Mechanics, because that whole science has been geared to linear clock time.

Hogarth I am not sure we are talking about the same thing. I'm just saying that we are in some sense physically bound by time.

Pask In what sense is one physically bound by time? I understand that we are physically bound by consensus and coherence. For instance, I can agree to watch a clock, and make correlations and measurements on that basis. But the type of event that goes on in a decision or learning process is not aptly tagged by this form of sequencing. You run into trouble if you imagine it is.

Atkin We are bound by events, and our sense of time is secondary to our experience of events. The problem is how to order the events in a way that we find useful. The standard time of Newtonian physics is sometimes useful, there is no doubt about it. The structural notion and the question of different dimensions suddenly becomes important when we ask what events are. If decision making means attaching yourself to an event, or trying to find a suitable connected chain of events, then decision making becomes the same problem as time.

Corkindale Much of the work under the heading of decision assumes that there are events, and that they are sharp and discrete. Although there are points at which things change in a catastrophic and dramatic way, most real life deciding is a continuous process. It is possible to "retrieve" situations.

Atkin This is because the situations are what I call p-events, where p is a big number and refers to the dimensionality of the event. Clock time is available to do the retrieving because that p-event has not yet been succeeded by the next p-event.

Nicolis Psychological descriptions of decision give the impression that we go from one state to another instantaneously. In fact there is a considerable amount of clock time in which you are not state identifiable —— disappeared in the ocean, as it were -- and you are really only identifiable at the events.

Pask Perhaps Mildred Shaw could comment on the way we structure decisions.

Shaw I have been working with Kelly's Repertory Grids. Basically this is a way of articulating constructs. They could be values, prejudices, emotions, books, facts, people -- it doesn't matter. It may be a way of articulating alternatives in the decision making process. Certainly it is possible to use this technique to develop your model on any topic, and then to compare it with someone else's model of the same topic. By considering the elements of your model in threes, and saying how two are alike, and thereby different from the third, one arrives at a set of bi-polar constructs on which all the elements are rated. All this has been developed as a program, PEGASUS, which, if you get two constructs which match, encourages you to add an element that discriminates between them. It can also exhibit to you the links you habitually make, but may not be aware of -- whenever you say"long"you also say "thin", or whatever.

Corkindale It has obvious implications for getting at objectives, the individual decision maker's criteria -- which are in fact the same thing from different angles.

Shaw By individual, I don't mean a body or a brain necessarily. I'm talking about perspectives in the same way as Gordon Pask. It could be an organization, or a part of you.

Corkindale In the clinical situation you are only interested in one subject and how he sees the world.

Shaw I would disagree with that. The point of psycho-therapy is to get all your P-individuals in conversation. The end point of successful therapy is to achieve communication between all the perspectives in your head.

Pask Its also the end point of successful creativity training. Synectics is one example, but they all do essentially the same thing. They cause you to adopt different perspectives and put them together simultaneously, often in a suprising manner. You then remove the part that is coherent in them all — if it exists. This can be done for individuals or for groups.

Shaw By starting with similarities and examining the differences you make channels of negotiation visible, where before there may have been no common ground. This is entirely content free, although the structure is determined. This is a new experience in interactive computing, because there is nothing there but the structure, and the content comes entirely out of you head.

M'Pherson It is interesting that this doesn't raise any arguments. When I talked about a structured decision analysis, which was content free, there was a huge argument. Why should you get away with it?

Pask Decision analysis works nicely provided you stick to the rules, but unfortunately it does not usually end up with a tree-like structure. You can persuade the client that this is how the world works, and that he ought to be coherent. He is allowed to be coherent in his own way, via a set of not-very-restrictive constraints called cluster analysis. Eventually you extract something from him on which you impose a tree-structure and use an elegant Bayesian inference or something. It is this piece of persuasion at the end where it all goes wrong. Up to that point its a wonderful technique.

Shaw I did not use a strict cluster analysis, but a matrix of ratings with an algorithm for matching constructs. All the analysis does is re-order the raw data to put like with like.

Atkin It decides on similarity by subtracting one number from another and deciding if it falls in a given range -- so its peculiarly attached to the idea of simple arithmetical difference.

Shaw I did it because.....

Atkin It works....

M'Pherson He means its not interesting to a mathematician.

Shaw Yes, it works. But the important point is to choose a form of analysis which does not obscure the original data, and does not produce complex computer output that baffles subject and experimenter alike. That automatically excludes factor analysis. I am trying Q-Analysis, but so far this is the most useful method to show the subject what he has communicated to the computer.

Atkin You are still inviting people to submit numbers, and appealing to their long miseducation about linearity.

Shaw At the moment we are trying to simultaneously elicit the topic grid and the constructs that show the way people are using the rating scale. One doesn't have to assume an ordinal 1 - 5 scale. People often use a logarithmic scale.

Atkin That doesn't matter. They are going to see the number four as the same as another number four. 2 is going to be nearer 3 than it is to 4.

Pask The integers are not that important. You could use the marks "circle","star","square","wildcat" and "reading lamp" just as well as "1","2","3","4", and "5".

Shaw The patterning is important, not the signs. With children we often use a colour scale.

Atkin I am suggesting we should not insist that the subject partitions his scale. He should be allowed to grade an item "1,2, or 3". Once you do that you have a relation not a mapping, and this gives a lot of structure. You could put your data on an incidence matrix, with (0,1) instead of integers, which would become the names of columns. If you did that you would be home and dry with a quick Q-Analysis.

Hogarth To return to content-free decision models, the trouble is that they are idealized processes, not really theories at all. In the Savage De Finetti sense, certain axioms are postulated, but no claims are made that people behave in that way.

Nowakowska Classical theory was too simple. It is essential to be able to test the axioms separately, and change them if necessary.

Nicolis If you perceive your environment as an object detached from you, then you can go on and apply decision theory. But if you see your environment as an I-Thou complex, you cannot apply decision theory. You can only decide.

Braten. It is very naive to assume that it is possible to build axiomatic theories, in the classical sense, which will allow the deduction or inference of valid images. The reference domain will interact with the source of the axioms, and transcend the system in which it works. The belief in axiomatic systems would have to be replaced by a belief in the competition between two axiomatic systems at least. You may have to go further and replace that belief with a new paradigm. Psycho-social systems create their own unique laws, and you can only search for general languages. The accurate articulation of laws and regularities at particular moments will depend on the needs of particular systems, within an evolving universe of new behavioural laws.

Nowakowska That is a meta-philosophy or a new epistemological level. I believe in models used as cognitive tools. I am not against so-called polymethodology that allows you to use different kinds of tools, simulation, dialectical thinking, and so on. But there is no better tool than a well-designed model that allows open experimentation.

Hogarth The trouble with models, especially economic models, is that their assumptions produce predictions which, in the aggregate, aren't too bad. At the moment I am working on a project to describe the way companies decide to relocate themselves, and global aggregates only work at their own level. The trouble is that the anti-economists have not produced alternative models that come up with better predictions.

Braten There is an explanation of that. For instance, in Norway the citizens and many of the governmental decision makers have been socialized to econometric models over a long period of time. Thus, gradually, Norway conforms to the patterns of econometric models. Eco-systems models are pretty new. We are not socialized to them and so we do not conform to the behaviour predicted by the models -- and thus they cannot be valid.

Thematic Summary

Foundational: the relevance of temporal assumptions to event description and decision.

problems of applying and validating models -- interactions with the referent domain.

Problems of imposing structure and metric; the relevance of Repertory Grids.

Section 9

General Discussion III

Discussion

Pask. This morning I would like to stimulate a discussion between Stein Braten, Maria Nowakowska, Gerrit van der Veer and Brian Lewis. Between them they have done more for action and value logics than anyone else in Europe, although they have different points of view. Maria is an axiomatist, with theories of pro-social behaviour, and of individual and group action. Stein takes a dialogical approach, Gerrit is oriented towards experimental psychology, and Brian is responsible for the Theory of Telling.

First, Gerrit van der Veer, who represents an extremely active group studying decision, education, and strategic learning. He would like to speak from that point of view.

van der Veer I must first say that I am a psychologist, not a decision theorist. I am working with "soft" data on humans in complex problem solving situations. This is not the same as taking a decision, but these kinds of real life problems incorporate systems of decision making. One task that we set is writing a computor program. One of the first things you perceive is that people disagree about whether a solution is the best solution -- or even whether it is a correct solution at all. We always find that everyone has his own style of solving these problems. If you ask questions -- "why did you got to the library and choose this kind of book?"; "why did you structure the subject in this way?" -- you find that subjects have many different interpretations of the goal, and are often not aware of the exact meaning of their own goals.

For instance, when you write a program, you can attach value to its speed; it should take the minimum amount of computor time to run. You can attach value to the transportability of the program from one machine to another. Alternatively you can say the program should be robust; you should be able to change the program. These different values and goals may be incompatible, although they needn't be. The thing that interests me is the possibility of teaching people to be better problem solvers in this sense. They should be aware of these different goal and value structures, and they should be able to reach agreement with each other on correct -- or at least valuable -- solutions. An important factor in this is the representation

structures that people use, whether in their heads, on paper, or in the form of a computor program. These different representations show the different values and goals they are using. Over the last two days I have tried to look at the kinds of analysis that would help with learning to learn -- that is structuring descriptions and values in a comprehensive way. Gordon Pask's method, the THOUGHTSTICKER system, is important. More than one person is working in the situation and they are obliged to make explicit their representation structures, not only of the world, but also of their goals and values. The video-reflection method also seems helpful in getting people to make their descriptions explicit, comparing them, seeing where they are inconsistent, and whether new values can be attached. I hope that, in bringing about higher levels of cognitive style, that these skills can be transferred and used in learning to learn, which must be a part of decisions.

Pask I am not yet sure whether decision skills can be transferred, but learning to make sense of an otherwise disorganized environment is undoubtedly a transferrable skill, if you recognise three things. First, this is an idiosyncratic process, whether it takes place in an individual or in a group or team. This brings in the larger issue of team selection, since people on the whole tend to plan and project in isolation. The effect of context, human or material, on learning style is an important question.
Secondly, it is necessary to have a fairly long exposure at any one session in order to achieve anything. It is no use playing around with one hour intervals. You have to work at it for a day or two, and then let something happen -- which is the third point. The effect is not immediate. We have found that many of our subjects have a latency period of between 2 and 6 months. At the time its simply an experience. Later they understand. The lowest component scores on their learning style profiles improve significantly, and their ability to reason by analogy increases.

It is not surprising in that sense. It is surprising as a quantifiable result. Most of the findings of conceptual and cognitive psychology are not surprising in that sense. Its simply that they hadn't been investigated, and only existed as apocryphal stories.

van der Veer The idiosyncracy of cognitive style shows even in simple tasks like paired associations. For instance, you give 25 ink-blot/letter combinations and ask people to learn them. Some take it as a rote learning task; others try to make visual images; others actively make verbal connections by telling stories using the picture and the letter. Not only did we find many different styles, but the subjects found structure in the material. We told them at the beginning that the experiment was to learn random combinations -- the letters and pictures had nothing to do with each other -- but the subjects simply didn't believe us.

Nicolis This is like the Bavelas experiments in Stanford. Subjects were put before a console with buttons and flashing lights, and told that by pushing certain bottons they could obtain a pattern -- and this would be confirmed by a buzzer. In fact there was no relationship, no wires at all between lights, buzzer, and buttons. Subjects worked very hard at this, some attributing exteremely sophisticated strategies to the machine. When the experimenter told them there were no connections, they were very upset, and often wouldn't believe it.

Atkin They might have been right. Your idea of randomness is only a disguise for what's really going on in some organizing scheme.

Pask This is something psychologists often don't appreciate. People are used to talking to people. People are complex and don't usually spew forth random noise and nonsense syllables, so they expect there to be some sense in machines. Also, I don't believe anybody ever learned a list of nonsense syllables. They learn a more or less sophisticated index, against which they place the nonsense syllables.

Lewis My work originates in the early 1960's, when we were building very expensive machines to teach people rather simple skills. I became interested in the possibility of teaching people by a simple process of one way communication -- essentially, telling them what they should do. This pushed me in the direction of ordinary language algorithms and error factors. There were four aspects of this: telling people what was the case, and indicating procedures to them (the prescriptive); and telling people what was not the case, and what they should not do (the proscriptive). That is an initial, very crude distinction, but the algorithms I developed

had a lot of success in industry, commerce, and medicine. The Tanzanian Health Service was basically established on the work I did in algorithms for medical diagnosis. One advantage of algorithms is that people are able to perform effectively without very much education. In Tanzania there are about a dozen diseases which account for 90% of all complaints. In each village, you train someone to give 12 tests. Are the patient's fingertips blue? Is the stomach swollen? Does he work near water? That sort of thing. If the tests are positive, then he prescribes. If they are negative, he calls in a "circuit rider",who goes round the villages, to give another set of tests. At this next diagnostic level, the qualifications are still less than those for a nurse. And so on. This works very well, and is about 50 times cheaper than our National Health Service.

A further advantage of this method is that it acts as a cost-effective training method. If you give people algorithms without understanding, then they become interested in why the algorithms work. This potentiates investigative behaviour, and can be very useful in the early stages of career training. A problem here is that algorithms threaten to demystify areas like medicine, and the medical profession is usually hostile -- initially at least. Eventually they are flattered, when they see a rather sophisticated skill is indicated, and this is roughly what they do. Although they claim to do other things as well, like "understanding the patient".

This work is difficult to describe briefly, but has important implications. I believe, for example, that algorithms provide the basis for a social ethic. If there is no algorithmic procedure for doing things, then you shouldn't criticise people for not being able to do them.
Then there is the phenomenon of error, which has always interested me more than truth. There is not much point in giving anyone an algorithm or rule unless you have some reason to believe they would go astray without it. Here I have tried to adapt Harlow's work on error factors in the Theory of Telling. You can only "tell" effectively if you can discover the underlying misconceptions that give rise to whole clusters of mistakes and inept behavious. When you tell people what to do, you are attempting to minimize the number of misunderstandings that can arise.

M'Pherson Are these algorithms restricted to simple problems ?

Lewis No. They have been applied in several complex situations. They work best in areas such as the handling of equipment, and Governmental rules and regulations. At that level, you can run into problems. I was commissioned to produce algorithms of Capital Gains Tax when it first appeared. The whole project had to be abandoned because the rules were manifestly inconsistent. You could switch the basis and do the calculation in a variety of different ways. At the social level, these inconsistencies formed the basis for Court actions, and at the theoretical level they raise the issue of specificity. If you are too precise, people can evade the law by producing categories that fall outside it. If you are vague, then it is not clear whether any judge would uphold your regulations. This problem of specificity runs through the whole attempt to algorithmicise any area whatsoever.

Johnson Your algorithms seem very similar to procedures for decision analysis.

Lewis Except for the problem of recurrence, which itself raises other problems. Obviously problems only recur with respect to the level of abstraction you have reached, but they must recur in order for it to be worthwhile to write an algorithm.

Braten This is one reason why, as social scientists, we will never be able to carry out experiments in any classical sense. I started out with the paradign that said it was possible to retrace sequences of human behaviour. This should be done by establishing a set of empirical procedures, specifying initial conditions, controlling some of the boundary conditions, keeping records of output variables, and so on. I did this with the moral dilemma processing dyads. My assumption was that I could create computor simulation records that would be comparable to each empirical case. I formulated a complex system that allowed symbolic worlds to be constructed in such a way that inferences were allowed and behaviours produced beyond the range of most axiomatic or symbolic systems. I did not expect first time accuracy, even with similar initial state conditions and variable values. I did expect that, after modification, the model empirical paradigm, realized as a simulation, would allow me to perform a complete classical experiment which I would not be able to do in the "real" world.

I tried out various structures and modifications to try and achieve a better match. Gradually, looking at the studies case by case, I found I was becoming clinical — being transformed from a scientist into a non-scientist. While the model could be made to match some cases, it could not be made to match others. Now I do not believe that it was possible to modify this model to account for all the different cases. There seems to have been an essential, almost catastrophic difference between the sub-sets.

This can be accounted for in two ways. Maybe I am a bad modeller. Or there may be competing patterns and regularities in the referent domain itself, and they cannot all be grabbed by one model. This leads to the attempt to construct competitors that would operate within one field, but the search would be for triggering or shifting mechanisms which would validate differing images. Obviously this will not be an hierarchical construction, but, as it has to contain shifting mechanisms, a meta-field cannot be avoided. The operational shifts must occur through the competitor's self-images in this meta-field. So there is an interaction between the field of models and the referent systems.

Now, what is the next step in this kind of reasoning? If we regard the realities we study as something of which we are a part, then this reality is capable of generating images of itself. This means that the referent system should be allowed to participate in the creation of images, and in transcending, retaining or destroying them. Some of these images will be in competition, and there will be shifts between operational modes and self-image generation through the meta-field. The basis of this is merely the requirement that each model, when in operation, shall produce its own record, or description.

This paradigm is intended to increase our repertoire of approaches in handling realities in terms of different kinds of descriptions; a search for a new kind of language to handle complementary and competing relations. Whether this language will be realizable in computer operational form, I do not know. In this respect, Husak's attempts to formalize the Hegelian Dialectic are interesting, and may open possibilities in the future. The task is made more difficult by the difference between our basic ontological assumption and the prevailing tendency, in symbolic logic and natural language, to think in terms of the dyad subject and predicate, or an actor and his actions. The adequate development of a language for competing relations and realities needs the assumption of

a dialogical reality in my terms. In Gordon Pask's terms, this would be called a conversational reality. The reality must not be monistic or monadic. Decisions are not made by one actor in vacuo. Consciousness must be regarded as impossible without a connectivity or intersection between at least two minds.

Nowakowska I admire Stein's courage in tackling these ideas, and I am not against them at all. They are very interesting and imaginative, although, in one sense they are very old. The problem is, in Hegel's time, there were no precise tools. I have taken that as a warning, and chosen to put my efforts in another direction, and into different kinds of models. I started by looking at similar types of communication problems but introduced multi-dimensional methods of communication — which I found easier than going into all these loops. I can cover the same ground by the so-called normal method of modelling. I introduce a relatively simple tool to cover such things as "one medium is inhibiting another" or "one medium is supporting another in a given meaning". There is an internal calculus covering the same aspects, but it is manageable.

My statement here will be very short because you all have the reports I brought with me covering three types of decision model. One -- Towards A Formal Theory of Dialogues -- is very general, covering internal dialogues with connections for the internal representations of how people talk about themselves. Another -- my Conference Statement — covers a special class of risky decisions. Here I have tried to show why some "suboptimal" decisions are in fact optimal, using special contextual interpretations of success and failure. The third is a theory of Prosocial Behaviour in the group situation. It assumes a typology of people, and attempts to show sequentially how people are influenced in their judgements and behaviour by others.

Although I was trained in dialectical methods, in practice I find them less powerful. What is known to date is not of that type, although it may be in the future. I hope something of this sort will be developed.

Lewis Are there any strong procedures for converging these dialogical models, or for making them self-correcting?

Braten There are no algorithms that can be followed automatically. Nor will you ever achieve a complete set of consistent statements to describe social realities.

Pask You could get strong procedures if you restricted the situation. But that misses the point that indeterminacy is inherent in situations involving human beings, especially when they are in complex decision situations. This can be illustrated by a device like John Nicolis' oscillator. I let it run, but cannot know if its iterations will reach any fixed point or limit cycle. If it does become recursive, if the iteration has a face, then you have a nameable state. In this case the procedure is strong, but you could not have foretold the result. Where humans are involved, where you have abduction as well as induction, strong procedures will only work if they include a representation which accommodates the actors. That is the nub of the problem for decision theory.

Thematic Summary

Foundational: the unpredictability of decision, Well-defined recurrent problems vs. ill-defined complex problems.

Decision as a choice among alternatives. Problems of discovering structures, and their idiosyncracy

Transferability of decision "skills", training and latency effects.

Ordinary Language Algorithms.

Section 10

Concluding Discussion

Concluding Discussion

The following questions can be used to orient our summary discussion:

1. What is decision making? How do you view the decision process and are there multiple perspectives?

2. What is a decision task and how can/should such tasks be structured?

3. The individual is often overlooked in analyses and theories of decision. How do you define the role of the decision maker? What systematic individual differences and variables should be included in a theory or representation of decision making?

4. Complementary approaches to improving decision include *training, aiding* and job/task design. What are appropriate "target variables" and how can they be evaluated?

5. How do you define "good decision" or "good decision making"? What are appropriate evaluation procedures?

6. What are the major unresolved issues in the development of descriptive (or prescriptive) theory(ies) of decision?

Johnson I am interested in these questions because they can be answered from many perspectives. In a very real sense, they are questions that people are answering every day. The major unresolved issues, for instance, are evaluated each time a research proposal is funded. At another level, graduate students have no difficulty at all in answering these questions.

Corkindale In terms of the evaluation of decisions and decision theories, I feel we are short of an adequate taxonomy. At the moment it is difficult to know if we are all taking about the same phenomena and the same set of variables. Here I think we have been talking more about individual decision making rather than decision in organizations. It is true that decision making is going on all the time, people are being trained in it, but there are no really satisfactory evaluation procedures of either. You can't assess the effectiveness of training until you have a taxonomy. Many models of training are based on a common element approach with real life. But until you have an adequate taxonomy, you cannot pretend to know what the common elements are, and how they are reflected in the training situation.

One way in which the individual is overlooked is in resistance to change. If you try and improve decision making, and introduce computors, or even something like Brian Lewis' Ordinary Language Algorithms, to any profession, they will feel that their status is threatened. It would be interesting to know if this was merely a matter of status and emotion, or whether there were genuine, but hard to describe variables involved that the theorists have not given due weight.

Hogarth With regard to the variables that define decision making and decision tasks, I would like to present a schematic system for thinking about these things. The scheme is derived from work I have been doing on how people learn relationships, and the sort of model one needs to understand the process. (Fig 5).

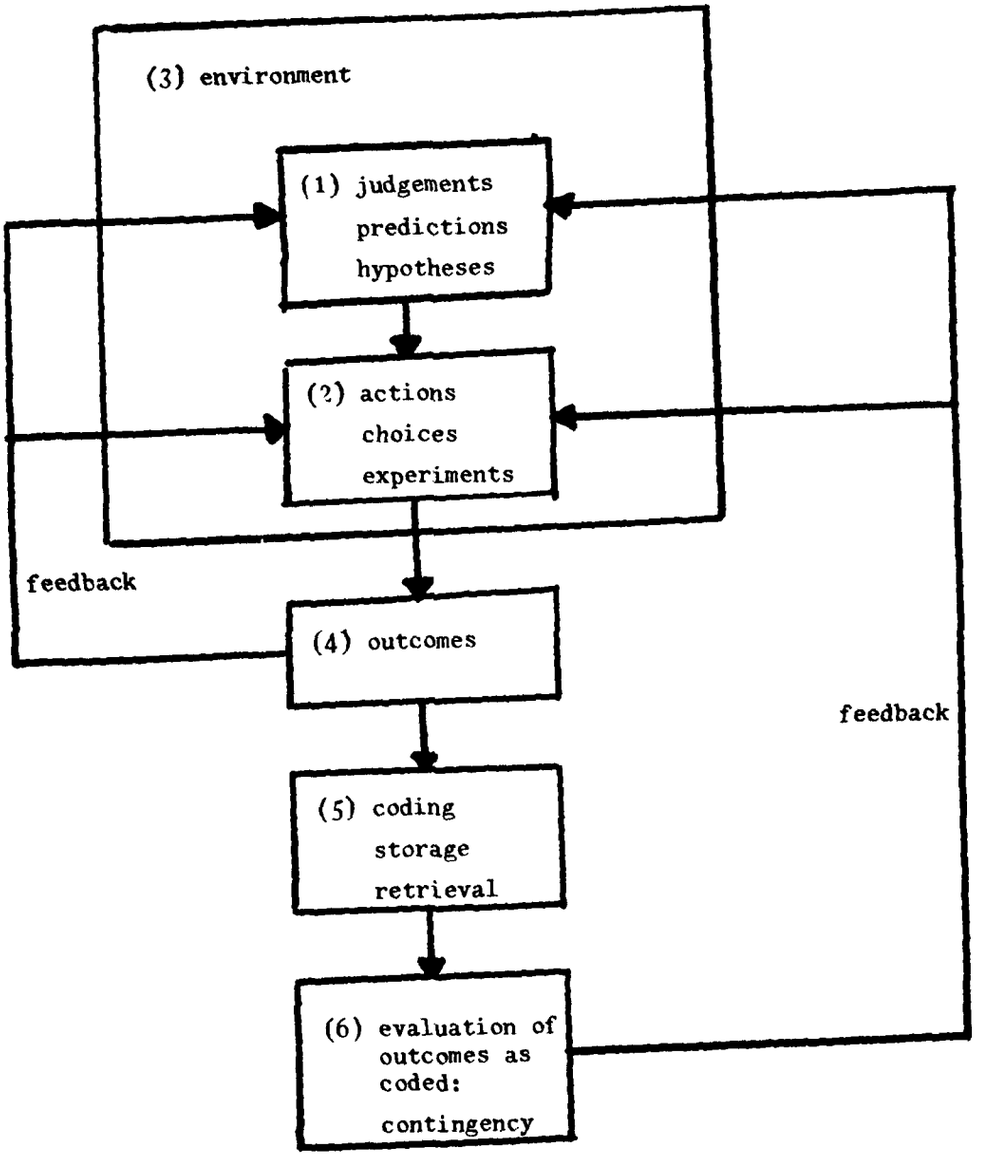
(3) environment
(1) judgements
predictions
hypotheses
(2) actions
choices
experiments
feedback
(4) outcomes
feedback
(5) coding
storage
retrieval
(6) evaluation of
outcomes as
coded:
contingency

FIG. 5

First of all people make judgements, or entertain hypotheses, within a specific environment. It is important to have a knowledge representation of the task environment both in its physical aspects, and as represented by the person. The judgements lead to actions, and this is where we locate decision making. The judgements and actions have outcomes, which in turn affect the environment and decisions made within it. This uses the idea of reciprocal determinism. It is not sophisticated enough to explain human behaviour in terms of the task, the environment, and the individual. You have to take into account the dynamic interactions among them.

Next the outcome is observed and stored, and subsequently evaluated. We need a model that can give an account of all these components. For instance, in the process of evaluation when learning about relationships, people are often more influenced by the absolute frequency (rather than the relative frequency) of events.

Nicolis Can this model accommodate conflict?

Hogarth Yes. In many ways.

Pask The boxes do not represent one perspective or one individual, but the system currently making the decision. Every box has the potentiality of internal conflict -- this could be added as a footnote to the diagram.

M'Pherson I operate within a fairly well structured world, so the answers to these questions are not too difficult. That diagram seems a good way of summarizing the decision making process. It must include all the prior analysis -- which in my world may involve man-years of forecasting and analysis. It must also include iterative cycles. The tendency to a single perspective seems to be the greatest deficiency of orthodox decision analysis.

A decision task involves the implementation of a system or organization, the creation of effective and efficient structures, through which the decision maker can operate. I am concerned with problems of the allocation of resources within large scale technological systems -- especially where cost-benefit analysis has broken down. The evaluation of medical screening programs is a good example of this.

Yes, the individual is overlooked. This is why straightforward decision analysis often has a hard time defending itself. The idea of responsibility is usually ignored. The decision maker, a politician perhaps, often has to act on behalf of a large spectrum of society, even of behalf of "nature". These people may not even know they are actors in the scheme. How do we get these complementary parties into the calculus?

The problem of improving decision making is to pursuade the decision taker to consider all the consequences within a consequence space, and to understand all the implications -- which may even change what he thought was the problem. The difficulty here is that the decision taker may be a very senior person, and there is a great imbalance between the man-years that go into the pre-decision task, and the time allowed to take the decision.

Defining a good decision obviously brings in ethics, and the possibility of reducing the mismatch between things as they are and as they ought to be. The mismatch ought to be reduced in such a way that no harm is uttered against individuals or nature.

Major unresolved issues are not so much in analysis, but in clarifying objectives. This means defining the problem by capturing the multi-attribute value-surface that includes the decision maker. When we look at questions concerning the unrolling of consequence, we have to ask how we should discount the future. Take the case of the Meadows disasters. They are barely interesting to decision makers, and are not allowed for, since they would change everything. What sort of things can be discounted when we are trying to control events and implement the future? Another unresolved issue is how to get politicians to have a time horizon that is related to actual problems not to elections.

Johnson This means that the analyst cannot be neutral, since he assumes his formulation of the time horizon is better than that of the decision maker. He may be formulating a different problem.

Corkindale This demonstrates the possibility of conflict within the boxes on the diagram. It also underlines that decision making is a process not an act. In this context its blindingly obvious that the importance of post-decisional processes has been ignored. The attention has been on a number of standard and useful pre-decision processes. Time scale and post-decisional process have to be very closely tied together.

Lewis When we come to defining decision making my worry is that these models may be premature, and aren't referred to a basis of observables. It may be that we need a far larger number of natural histories before we can construct models.

M'Pherson One great problem with modelling is to maintain a spirit of scepticism. There is a natural tendency to assume, when you have worked with a model for a year or so, that it isn't a model at all — it is a world.

Robinson This may not be the epistemological disaster that it appears. Brian Gaines pointed out that the majority of real, complex decision problems tend to arise in areas that we have created -- usually technological or informational. It may be that these areas are genuinely unstructured with respect to the possibility of enacting meaningful decisions. If that is the case, then the imposition of a model may be necessary before anything can be done. This complicates the problem of distancing yourself from the model in order to evaluate it.

Lewis And brings up the question of evaluation. This is meaningless to me without the notion of a purpose and a point of view. What is good for the country in the short term isn't necessarily good in the long term. When people are induced to go to war, its hardly good for the individuals who get killed. Ethics is important, but it has a tendency to stand in the way of an analysis of consequences. For example, at the Open University

we have the choice of assigning tutorial effort to those students who are struggling, or to those who are potential geniuses. There are not the resources to do both -- or there would be no conflict. The problem is that people immediately move into sloganizing. Instead of analysing consequences, they re-iterate "we are a second-chance University" and so on.

Braten I found great difficulty in answering these questions because they imply a certain manner of slicing up the world; so I have tried to answer them in terms of the scheme I employ in computor modelling various psycho-social systems. Briefly, I shall describe this way of looking at the world. (Fig 6)

A distinction is made between intersecting or interacting fields. The first field is capable of containing, or allowing for the generation of action programs. Potentially, this may be described in terms of variety in Ashby's sense, or — if one goes a bit further — in terms of organization. The second interacting field is called the orientation field, and may allow for meaning-tightness, or meaning-tight kinds of description. It contains the contents of communication, interpretations, selected message contents, and so on. The third field is called the material signal field, and may allow for energy tight descriptions.

From Gordon Pask's Conversation Theory, and my own Dialogic, we may also say that all processes involve at least two participants in some way or other. We therefore have to distinguish between the innenwelt or internal environment of each participant and the umwelt or external environment. In the former, the participants generate and make use of elements and relations between elements, activating some and ignoring others. These processes may run in parallel or be disjoint, and may do so within an individual, between individuals, in groups, social organizations, and societies. These processes take place in relation to each other and in relation to a common umwelt -- the common world in which they materially affect each other by action and which constitutes sources of raw data to which they assign meaning.

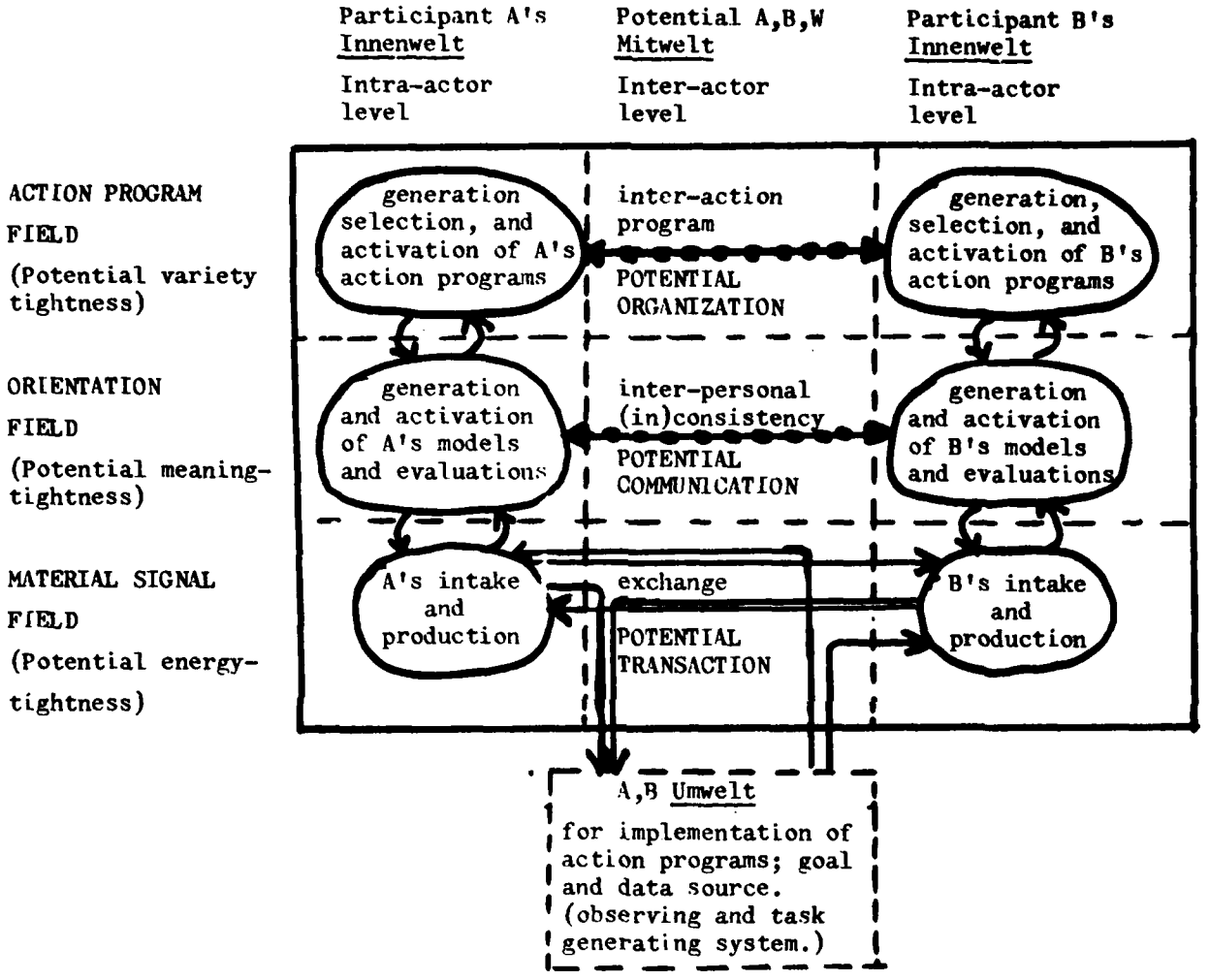
Participant A's Innenwelt
Intra-actor level
Potential A,B,W Mitwelt
Inter-actor level
Participant B's Innenwelt
Intra-actor level
ACTION PROGRAM FIELD
(Potential variety tightness)
ORIENTATION FIELD
(Potential meaning-tightness)
MATERIAL SIGNAL FIELD
(Potential energy-tightness)
generation selection, and activation of A's action programs
inter-action program
POTENTIAL ORGANIZATION
generation, selection, and activation of B's action programs
generation and activation of A's models and evaluations
inter-personal (in)consistency
POTENTIAL COMMUNICATION
generation and activation of B's models and evaluations
A's intake and production
exchange
POTENTIAL TRANSACTION
B's intake and production
A,B Umwelt
for implementation of action programs; goal and data source. (observing and task generating system.)

FIG. 6

This way of making distinctions in modelling social systems can be extended to the processes involved in decision making. Decision making will be defined as concerned with processes in the mitwelt -- the potentiality of, the creation and subduing of common points in these different fields at the interactive level. The problem for us as participants or observers is that the three fields call for different approaches and different descriptions because they are concerned with different processes and relations.

The creation of structured organization will certainly involve conflict, since it is not merely the action programs of A or B, but some interaction program for a set of interaction programs. The creation of meaning-tightness by communication concerns the relations between images and relations reflecting the interests and perspectives of A and B. This becomes problematic in terms of decision making since the material space does not allow for the concurrent implementation of A's and B's programs. It does not involve organization or communication but the possibility of transaction in the material sense. So this scheme involves organization, communication, and transaction.

The problems of decision making concern not only potential conflict in the different fields, but the possibility of the degeneration of the fields themselves. In practice this must involve conflict. In theory it means, at least, that key variables are ignored. Potential dialogue is under threat of becoming a monologue. Communication may become persuasion. Exchange easily becomes exploitation.

This allows for a partial description of the role of the decision maker. He must establish some kind of organization, and a form of communication that allows anticipatory simulations of differently generated programs. He must create forms of exchange that allow for the implementation of these programs without destroying the data sources, thus leaving open a further series of organization, communication, and transaction.

O'Shea I find these questions alarmingly hard to answer. In all of them you can delete "decision-making" and replace it with the name of any other cognitive skill, and the question is just as hard. The only non-tautological answer I can give is on unresolved issues, and that is simply "how do we use what we know?".

van der Veer We can use what we know to create algorithms, but when the algorithm fits you are no longer in the position of the decision maker. In this sense, I agree with the epistemologies that have been suggested. I would like to add that I can't define a good decision, but I would like to be able to say how you define better decisions. Each decision is good as long as all the persons involved agree. But when the situation changes, or another person comes to the group, there may be a better decision.

Robinson Decision making itself can only be defined by highly abstract concepts to do with the dialectics of consciousness, and these necessarily involve multiple perspectives. Specific decision tasks can be resolved -- made obsolete in John Nicolis's terms, and the taxonomy I am attached to is that of externalization, reification, and reintegration. Obsolescence of the problem is the sign of successful reintegration.

The notion of individual in the classical sense has very little relevence to decision making.

Target variables must depend on specific theories and problems. I would like to know how many levels of image holding are both possible and useful. Stein Braten's self-reflective groups showed at least three levels of image holding, of which only two seemed useful. In my own simulation studies of sm[illegible]group behaviour, there were many situations in which only one lev[illegible] image holding was necessary and useful. At the other extreme, with Governmental decision making, I can't even begin to characterize the number and levels of images. Yet decision-aiding must involve the augmentation of this ability, and so the job will have to be tackled.

As far as "good" decisions go, the criterion must be relative, and very much derived from an analysis of perspectives. We agree that the decision to implement the first production line technology was good historically. It was also a very good decision as far as the factory owners were concerned, but it was usually disastrous for the artisans that were put into the factories. I don't believe that such conflicts can be theoretically resolved, and I am certain that they cannot even be approached with static (and that includes"ethical")schemata.

Pask I think you all know what my answers will be, so I will be brief. Decision making is the whole gamut of activity that has been so ably described today. There are and there must be multiple perspectives in order to have decisions at all. These could be different individuals

or perspectives entertained by one individual. Individuals are not very relevant in this sense because,when they engage in activity, perspectives tend to become distributed. Individual differences, if one recognises that they are not context free, are important — especially at the level of assembling teams.

There are many ways of training and aiding decision, and I believe that the closer to the job the "training", the more effective it is. I don't know if its possible to "teach" decision, but I am certain, as Donald Broadbent commented at the last Conference, that you don't teach it by teaching a load of equations.

Good decisions must be defined in context. Good decision making involves the ability to abduce and resolve juxtaposed abductions, and the ability to maintain the variety of the system while avoiding chaos.

The major unresolved issues are legion.

Thematic Summary

Foundational: decision theory as based (at least) on interacting perspectives

Orienting Questions: structure, value and evaluation of decision and decision theory; role of the individual; unresolved issues and target variables

The importance of the decision maker: his relation to his environment

The role of ethics in decision making: the relativity of a "good" decision

Decision process taxonomy